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Philosophy of Science

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Are There Ultimate Simples?

BY

JULIUS R. WEINBERG

IN THE course of modern philosophy, there have been several attempts to demonstrate the existence of ultimately simple objects by purely logical methods. One of the most recent of such attempts forms part of the foundation of Wittgenstein's logical doctrine. As Wittgenstein has, until quite recently, been considered the authoritative source of Logical Positivism, an examination of his supposed demonstration of logical simples is propaedeutic to an evaluation of the method of the school.

The doctrine of logical simples is fundamental to the philosophy of Logical Positivism. The two principal aims of that philosophical movement are: to present science with a secure logical foundation, and to eliminate metaphysics from all significant discourse.[1] In order to do these things it is necessary to show by logical analysis that there is only one complete analysis of any given proposition, and that this analysis is absolutely terminated in the discovery of elementary propositions which are directly and solely concerned with immediate experience. The doctrine that ultimate simples exist is required to insure the completeness and uniqueness of any given analysis. The doctrine can either be assumed, or demonstrated, or introduced as an arbitrary

[1] Wissenschaftliche Weltauffassung, Der Wiener Kreis, Wien, 1929, p. 15 ff.

postulate (convention) to be justified by the success of the system employing it, but it is evidently essential to Logical Positivism. For we can see the possible consequences of logical analysis in case it is not introduced.

To found science on a logical, empirical basis and to eliminate all trans-empirical concepts as being non-significant, it is necessary, as remarked above, to prove (1) that there is only one complete analysis of a proposition and, (2) that this analysis has a theoretically ultimate limit in empirical reality. Now, if many distinct analyses were possible, or if there were no limit to a given analysis, then the theses of Logical Positivism could not be demonstrated. Even though a given analysis of propositions reduced them to propositions exclusively concerned with empirical reality, another possible analysis might very well lead to propositions which were not exclusively concerned with the empirical reality. Similarly, if there were no ultimate limit to analysis, although a given analysis led solely to the empirical reality when carried out to any previously assigned point, further analysis beyond this point might reveal non-empirical reality. Both of these apparent possibilities have to be excluded.

The doctrine of logical simples implies that there is only one complete analysis of propositions, and that this analysis terminates in the empirically grounded elementary propositions. There are three definite steps which conjointly establish the thesis. These are: (1) the demonstration that any proposition is a truth-function of the elementary propositions, and conversely that from any set of elementary propositions, a given truth-function can be constructed; (2) the demonstration that apparent exceptions to this (1) do not exist; (3) the demonstration that the elementary propositions entail logical simples.

Propositions ostensibly compounded out of other propositions are always truth-functions of the latter. Consider the proposition P which is an ostensible compound of p, q, r, ... P is either a truth-function of p, q, r, ... or it is not. If it is a truth-function, it can be reduced to p, q, r, ..., and conversely from p, q, r, ... P can be constructed. Wittgenstein accomplishes this by an amazingly simple, yet very effective device.

The truth-connectives holding among any group of propositions are: "not," "and," "or," "implies," "is equivalent to," "not both," "neither—nor," "one but not both," "one and not others," etc. Now all of these can be defined in terms of one and the same operation. We may choose "equivalence"[2] or "mutual rejection"[3] or "incompatibility"[4] as the primitive operation. Wittgenstein chooses mutual rejection and defines all the logical truth-operations as compounds of mutual rejection or the "stroke," "/". Thus P/Q is "neither P nor Q." P/P then becomes "not-P," "P or Q" becomes (P/Q) / (P/Q), etc. In general, then, any truth-function of the elementary propositions is a successive and varied application of '/' to the elementary propositions.[5] The demonstration that all propositions are truth-functions of elementary propositions then becomes quite simple. At this point an elementary proposition may be considered simply as one which does not ostensibly contain other propositions, whereas a non-elementary proposition is one which does ostensibly contain other propositions.

Now if all the truth-functions are variations of one and the same function, namely the stroke, it is clear that any non-elementary proposition is a compound of other propositions formed by means of the stroke and can be reduced to elementary propositions simply by omitting the principle strokes, then the strokes of lesser scope, etc., until the elementary propositions are obtained.

There are certain apparent exceptions to the assertion that propositions occur in others only as bases of stroke-functions.[6] For this assertion is equivalent to the doctrine that there are only finite truth-functions, i.e., extensional functions, of propositions, whereas it seems as if other functions of propositions might

[2] Tarski, A., Sur le terme primitif de la logistique. Fundamenta Mathematicae, Vol. 4, pp. 196–200, 1924.

[3] Sheffer, H. M., Trans. Amer. Math. Soc., Vol. 14, pp. 481–488, 1913.

[4] Nicod, J., A reduction in the number of primitive propositions of logic. Proc. Camb. Phil. Soc., Vol. 19, pp. 32–41, 1917.

[5] Wittgenstein, L., Tractatus Logico-Philosophicus, London, 1922, prop. 6.001.

[6] Wittgenstein, op. cit., prop. 5.54: "a proposition occurs in another proposition only as the base of a truth-operation."

occur. In particular, it seems as if infinite truth-functions and intensional functions might occur. For example, the idea of universal quantification, i.e., "for every X, ØX obtains" does not seem to be developable from a set of elementary propositions by means of the stroke. There would then be infinite truth-functions which could not be reduced to elementary propositions. Again, the occurrence of a proposition P in the assertion "P contains an apparent variable" seems to be a case of P's occurrence in a larger proposition other than as the base of a truth-operation. The same obtains for "John believes P."

Wittgenstein, to whom this whole analysis is due, has shown that these apparent exceptions cannot stand the test of analysis. Universal quantification is an ambiguous idea, which when rendered unambiguous, does not constitute an exception. For a finite number of cases, "for every X" becomes equivalent to "this X and that X etc." An infinite number of cases of propositions does not occur because the idea of infinity is not applicable to propositions. It is not, in fact, a logical idea at all, but belongs rather to mathematics.

The intensional function remains to be dealt with. A propositional sentence has two aspects between which Wittgenstein radically distinguishes. As it is spoken, thought, or written, it is a series of noises, a psychological process, or a series of marks. It is, that is to say, a fact in its own right. As it is used to represent some fact, no account is taken of its own factual nature, but, by means of it, another fact is represented. Now it is easy to see that it is only with the latter aspect that we are concerned when we speak of "the proposition" or "the symbol" in its occurrence as a part of another "proposition" or another "symbol." Hence P, as proposition, is intended when we say that P occurs in a truth-function, whereas, P, as a fact, is intended when we say "P contains an apparent variable" or "John believes P." These two aspects of the propositional sentence have not the least to do with one another. The "intensional" exceptions do not actually constitute genuine exceptions.[7] From these considerations we are naturally led to the idea that there are only two kinds of

[7] Wittgenstein, op. cit., prop. 5.542.

propositions, elementary and non-elementary (the latter being simple truth-functions of the former), and further that the elementary propositions constitute the ultimate basis of all significant discourse. It could, of course, be urged that the elementary propositions might themselves be functions of other propositions. But, while this view does not lead to contradictions, it produces practical difficulties which are equally vicious. Assuming that the number of propositions is definitely limited, the truth of a given proposition would then become dependent upon itself, since it would become, in part at least, a truth-function of itself. On the other hand, if no limit to the number of propositions is assumed, the truth of any proposition would depend on the truth of others in an unending regress. In this case, the truth would never be finally determined.

In other words, since the truth of a compound proposition depends upon the truth of its constituent propositions, there must be some proposition whose truth depends upon agreement with reality rather than upon the truth of constituent propositions. Otherwise, truth would either be dependent on itself or upon an infinite regress. Hence, if truth exists at all, there must be elementary propositions. This is a final conclusion to which almost everyone would give assent. And if this were the whole of Wittgenstein's argument, no difficulties would arise.

Wittgenstein, however, believes that the nature of elementary propositions requires the assumption of logical simples. It is this part of his doctrine that requires examination. The argument purporting to demonstrate the existence of logical simples runs as follows: "If the world had no substance (i.e. simple objects) then whether a proposition had sense would depend on whether another proposition was true. It would then be impossible to form a picture of the world (true or false)."[8] The significance of this argument depends on the explanations of terms given in various parts of the "Tractatus." The principal terms requiring explanation are "object," "fact," "proposition," "true and false," and "sense of a proposition."

A proposition always enunciates the existence or non-existence

[8] Wittgenstein, op. cit., prop. 2.0211–2.0212.

of a fact. Logical analysis should find its ultimate data in elementary propositions and the facts which they represent. Names and other ingredients of symbolism interest the logician merely as material for constructing propositions and not on their own account, because names, etc. have their sole significance as articulate parts of propositions. The same consideration obtains for objects and facts. Objects as such are purely formal entities which constitute the constituents of facts. They can only be conceived in the context of some facts or others. On the other hand, some description of objects and names is necessary in order to understand the nature of the facts and propositions constructed out of them. An object may, therefore, be described as whatever is a distinguishable part of one or more facts. A fact is simply a combination of objects. This circular description is unavoidable and does not, it is admitted, explain very much. It does serve to point out the independent and complete character of the fact as contrasted with the dependent and incomplete nature of objects. The proposition has already been explained as a picture or representation of a fact. If the fact exists, the proposition is said to be true, and if not, false. The sense of the proposition is what exists if the proposition is true. Obviously, a false proposition can have sense. These explanations suffice for the discussion of Wittgenstein's argument.

One further principle is required. "Every statement about a complex can be analysed into a statement about its constituents and into those statements which completely describe the complex."[9] This seems to mean that whenever a statement occurs in which there is the name of a complex, a translation of that statement into another statement (or several other statements) is possible such that the name of the complex gives way to a concatenation of the names of its constituent parts. By virtue of the considerations in the third paragraph of the present essay, one and only one analysis of a given complex is possible.[10] We may now consider the demonstration that simples are necessary. Wittgenstein supposes that the assertion that a proposition has

[9] Wittgenstein, op. at., prop. 2.0201.

[10] See also Wittgenstein, op. cit., prop. 3.25.

sense is incompatible with the assertion that the proposition contains names which are reducible without limit to other terms. This is clear from the direct form of the demonstration. In the indirect form given in the Tractatus the argument is: If the world has no simples, then, if any proposition has sense, another proposition is true, and if any proposition has sense implies another proposition is true, then it is impossible to form a picture of the world, i.e. no proposition has sense. In the direct form: It is possible to form a picture of the world implies it is false *that any proposition has sense implies another proposition is true*, and if it is false that *any proposition has sense implies another proposition is true* then the world has simples.

We may now elucidate the argument. If the world had no simples then propositions containing names of complexes would be transformed into other propositions asserting that the elements of the complex were united in such and such a way. These elements are *ex hypothesi* not simples but complexes in their own right. The sense of any one proposition about complexes would depend on the truth of those propositions into which it would be transformed. This process of transformation would proceed without end. Hence, there would be no connection between the propositions and their extra-propositional referents. This implies that no connection between the pictures of the world and the world itself exists. Hence picturization of the world is out of the question and this is equivalent to the assertion that propositions have no sense. The denial of simples is, therefore, logically incompatible with the assertion that propositions have sense.

In the direct form, the argument has the form: If propositions have sense independent of other propositions, then there are simples. It remains to ask whether this is a demonstratively certain proposition. If there is a logical connection between propositional sense and the existence of simples, the argument is a *petitio*. If not, the argument is an *ignoratio*.

A consideration of what Wittgenstein means by the sense of a proposition shows that the argument is not an *ignoratio elenchi*. The sense of a proposition is the possibility that the fact to which it refers (i.e. the fact which it pictures or represents) exists. In

order that a proposition have sense, it is necessary to establish a bi-unique isomorphic correspondence between the proposition and its referent. That is, the terms of the proposition must be correlated with the elements of the fact, and the form of representation of the proposition must be identical with the structure of the fact. In other words, in order to have representation of one thing by another there must be an identity of the arrangement of the elements of the one and the arrangement of the elements of the other and a one-one correlation of the elements of these respective things.[11] From this it is clear that the sense of propositions is essentially connected with the nature of facts. The argument is, therefore, not an *ignoratio.*

But, inasmuch as the kind of correspondence required in Wittgenstein's conception of sense is possible only on condition of a correlation of simple names, simply arranged, with simple objects, simply arranged, the conception of sense implicitly contains the assumption of simple objects. In other words, what is to be proved has been implicitly assumed, because what has to be proved is the existence of simples which depends upon the existence of a proposition with a significance independent of other propositions, and this last depends upon the existence of simples. Hence, the argument is a *petitio principii* in the form in which it was given.[12]

We are now prepared to consider whether the argument can be construed so as to avoid the trivial appearance that it has presented thus far. Or rather, let us inquire whether there is any other argument in the Tractatus, explicit or implied, that would necessarily establish the existence of simples. There seems to be such an argument, but it can be shown to be quite inadequate to demonstrate what it attempts to demonstrate.

We turn to the definition of fact given in Wittgenstein's work. We found that the argument for simples depended upon 'sense.' It was shown that 'sense' in such an argument was either irrelevant

[11] Tractatus, 3.21, 2.13, 2.15, 2.18, 4.0312.

[12] It is true that all formal reasoning is a begging of the question if something about the world is in question. To say that a reasoning is formally rigorous is, therefore, to deny that anything about the world has been established thereby.

or arbitrarily defined in lieu of further information. Perhaps the conception of 'fact' will supply this need, and will show that only one conception of 'sense' is possible, because only one conception of fact is possible. This, if true, would remove any appearance of triviality, and would present at the same time an independent argument for the existence of simples.

"A fact is a combination of objects (entities, things)."[13] There are, of course, facts (Tatsachen) composed of other facts, but these divide into independent facts (Sachverhalte), and we shall henceforth be exclusively concerned with the latter. A fact, then, is not a simple entity, but always a complex of objects. Objects, on the other hand, are conceivable solely as possible constituents of facts. The objects contain the possibility of those facts of which they are possible constituents. In the fact there is no further entity by virtue of which the objects are, as it were, held together. The objects simply combine immediately with one another to form a fact, and this combination is internal to the objects. The objects, that is, "hang into one another" in the fact. The specific mode of combination, the way in which the objects hang together, may be called the structure of the fact.

What, now, can be deduced from the definition given? If a fact is a combination of objects, it is necessary that it be a specific combination in order that the fact be a specific fact. The combination will be singular, since it is necessary only to have *one* mode of combination of two or more objects in order to have *a* fact. Hence, the simplest kind of facts conceivable will be singular, specific modes of combination of objects. Furthermore, in order that the combination of objects in the fact be singular and specific, it is necessary that the objects be irreducibly simple, for a singular and specific mode of combination could not obtain among objects which were complex. A singular, specific mode of combination among complexes is a contradiction in terms. If, then, there are facts in the sense defined, there will necessarily be irreducibly simple constituents of facts. And, of course, if there are not facts in the sense defined, it is difficult to conceive what kind of complexes facts may be. Facts, therefore, are either

[13] Tractatus, 2.01.

definite or ambiguous combinations. Wittgenstein would unquestionably regard an 'ambiguous combination' as a contradiction in terms,[14] and it is evident that a 'specific singular combination among complexes' is a contradiction in terms. Hence, if there are facts, there must be specific facts, and if there are specific facts, there must be absolutely simple constituents of facts. This, apparently, is the argument which underlies the one which we considered above. We may, therefore, consider it exclusively, and inquire as to its validity.

It is, evidently, an argument from definition. As it stands it is formally valid, but still trivial, because purely verbal. In order to render it free from this charge, it would be necessary to show: 1. that it is really possible to define what is meant by 'a fact,' and 2. that the definition which has been given in the Tractatus is the only legitimate definition possible.

Let us consider (2) first, because it leads into (1) more understandably than in the logically more correct order of these requirements. How could we demonstrate the exclusive correctness of the definition? There appears to be only one way, namely, the method of intuitive induction. Just as we grasp the essence of triangularity from an examination of several triangles, so we should be able to grasp the essence of 'fact' from an examination of several facts. If by this method we find one fact that satisfies our definition, we know that the definition contains a sufficient condition of 'being a fact,' and if we also grasp the one and only essential of 'being a fact' from an examination of several facts, and find that this essential is in our definition, we also have the necessary condition.

The successful application of intuitive induction in this case depends (1) on whether there is a common characteristic of facts in virtue of which they are facts, i.e. on whether all facts share an essential characteristic called 'being a fact,' and (2) on whether, assuming that there is such a characteristic, it can be introduced into discourse as the definiens of a definition.

This remains an open question since there are no known argu-

[14] Tractatus, prop. 5.5423: "To perceive a complex means to perceive that its constituents are combined in a definite way."

ments to support an affirmative answer and many that support a negative one.

Principal among the arguments on the negative side is the difficulty of conceiving how facts which have neither constituents nor components in common could have anything in common. If, e.g., fact F′ has a, b, etc. as constituents and R as component, and F″ has m, n, etc. as constituents and S as component, F′ and F″ have nothing specific in common. What is the justification for calling a, b, m, n, by the common name 'object' and R, S by the common name 'structure?' In order to construct a definition it is necessary to use these common terms, but the terms have no apparent connotation, since they denote things with nothing in common.[15] If these terms have no connotation, they cannot be used in a definition that must present an essential characteristic. And, in another place, Wittgenstein, himself, has said that 'object' is a pseudo-concept and occurs in discourse solely as a variable name.[16] It seems reasonably clear that this renders definition of 'being a fact' impossible, and it throws considerable doubt on the existence of this essential characteristic.

Intuitive induction may discover the essence of 'being a fact,' but this essence cannot be expressed. As a consequence, the argument from the definition is meaningless. That is to say, if the definition cannot be given, deductions from it cannot be given. If it is necessary intuitively to grasp what it is to be a fact, then it is also necessary intuitively to grasp what it is to be a simple constituent of a fact. There would then be no argument for simples, but everything of this kind would have to depend upon intuition.

If, however, it is possible to define fact, if the use of the words 'object,' 'structure,' etc. do have some connotation, it is still difficult to see how the definition could be justified. How, in other words, assuming *some* definition to be possible, could we prove that one particular definition alone was possible? Intuitive induction would not serve us in this case. We can conceive of many definitions of fact which are not self-contradictory. It

[15] Equivocation in Aristotle's sense. Cf. Categories 1.1cc.

[16] Tractatus 4.1272.

seems quite impossible to demonstrate that one alone is correct. No logical principles would serve to do it. Direct examination of facts will not suffice. Experiment and problematic induction are obviously useless for such a task.

We are forced to conclude that the possibility of defining 'fact' is questionable, and that even if we disregard this questionable point and proceed to construct a definition, there is no way to demonstrate the exclusive correctness of it. Other definitions are always possible, and there will be no way to judge which among them is exclusively correct. We can make deductions from these definitions, but since the definitions are quite arbitrary, deductions from them will have merely a verbal force.

What, then, must be said about the possibility of demonstrating the two principal theses of Wittgenstein's Logical Positivism? First, can the exclusively empirical reference of all significant discourse be demonstrated by any logical methods? Secondly, is it possible to demonstrate the ultimate empirical reference of discourse without self-contradiction? This question cannot be adequately treated here. The contradiction arises because after we have demonstrated that the ultimate reference of discourse is exclusively empirical, we are forced to account for our demonstration which obviously cannot itself have an empirical reference.

At first sight, it would seem possible to show that, even without the apparently indemonstrable doctrine of ultimate simples, propositions must be possibly true or false in order to have any significance, and that the only decisive criterion of truth or falsity is an experiment.

But that an experiment is an exclusively empirical test remains to be proven, and cannot be assumed or arbitrarily introduced as being empirical by definition. The elements of theory and implicit beliefs which cling to every experiment in natural science have to be eliminated by some method or other. This would be particularly hard to accomplish in the case of a microscopic experiment. It frequently happens that we think we have verified a proposition, when we have really only confirmed the consequence of a theory. It is such a case as this one which should make us

extremely skeptical of equating experimental and empirical criteria of meaning.

The argument that the "sense" of a proposition in the widest sense of the word must ultimately be determined by extra-propositional considerations (i.e. the facts, whatever they may be), is not subject to the same criticisms leveled against Wittgenstein's particular argument. It is clear that any infinite regress in which propositions always depend on other propositions in respect either of meaning or truth leads to absurd results. This merely establishes the fact that propositions have an ultimately extra-propositional reference. From this, nothing can be determined concerning the nature of this reference. This argument does not suffice, therefore, to prove the empirical character of propositional meaning. The stronger argument of Wittgenstein would, if it were valid, suffice to demonstrate that conclusion. But, as we saw, this was not the case.

The only safe judgment in regard to this matter at the present stage of logical investigation seems to be: That some propositions have not been shown to have an exclusively empirical reference and that, consequently we do not have the right to regard metaphysics as meaningless, because non-empirical, until we have shown that every non-metaphysical proposition is purely empirical.

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On the Problem of a Unified Physical Theory of Matter

BY

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IN THE following, limiting ourselves to two objects—the processes X and Y—we will compare three kinds of regularities in their specific manifestation in physics: (1) interaction; (2) causality; and (3) functional dependence. In considering as objective all the regularities which are inherent in things and material processes themselves, and in considering causality and functional dependence merely as one-sided abstractions of interaction, which in its turn is an abstraction from the universal interconnection of things, we avoid such an arbitrary definition of causality as, for instance, the possibility of exact prognosis[1] which leads to the negation of causality. We also avoid placing functional dependence above causality.

(1) *Interaction* of the processes X and Y, which we will symbolize as $X \leftrightarrow Y$, is characterized as follows:

a) X and Y are connected by logical necessity;

b) X and Y are definitely connected, which is usually expressed thus: the action X and the action Y are mutually equal. The

[1] Cf., for instance, *Max Planck "Die Kausalitat im Naturgeschehen,"* "Scientia" of March 1, 1933, where he says: "*Ein Eregnis ist dann kausal bedingt wenn es mit Sicherheit vorausgesagt werden kann,*" and where, proceeding from mechanistic materialism, Planck arrives with logical consistency at the recognition of an omniscient absolute spirit.

principle of conservation and transformation of energy is a present day formulation of this.

c) the connection $Y \leftrightarrow X$ is equally true;

d) the connection includes in itself no statement as to time (neither of succession nor of simultaneity);

e) the connection includes activity, effective, active compulsion;

f) the connection is transitive, i.e. if $X \leftrightarrow Y$ and $Y \leftrightarrow V$, then $X \leftrightarrow V$ is true;

g) the connection is but a link in an endless chain: $(\ldots. \leftrightarrow Z \leftrightarrow X \leftrightarrow Y \leftrightarrow V \leftrightarrow \ldots.)$.

(2) The causal connection of the process X (cause) and the process Y (effect), which we will symbolize as $X \rightarrow Y$, is characterized as follows:

a) X and Y are connected by logical necessity;

b) X and Y are definitely connected, which is usually expressed: the cause is equal to its effect;

c) the connection is asymmetrical, i.e. $Y \rightarrow X$ would not be true;

d) the connection includes a succession in time, i.e. X precedes Y in time, which we will symbolize as $X \circ\!\!\rightarrow Y$;

e) the connection includes activity, the process X represents the active principle and the process Y the passive principle; it includes effective active compulsion;

f) the connection is transitive, i.e. if $X \rightarrow Y$ and $Y \rightarrow V$, then $X \rightarrow V$;

g) the connection is but a link in an endless chain: $(\ldots. \rightarrow Z \rightarrow X \rightarrow Y \rightarrow V \rightarrow \ldots.)$, whence follows the relative character of the concept of cause and effect, for each of the links in this chain represents both a cause and an effect;

h) the connection reflects only one-sidedly the real interaction of $X \leftrightarrow Y$, due to abstraction from its co-existent (in interaction) connection $X \leftarrow Y$.

(3) Functional dependence, which we will consider in the most abstract mathematical form of $Y = F(X)$ is characterized as follows:

a) X and Y are connected by logical necessity;

b) X and Y are definitely connected;

c) the connection $Y = F(X)$ allows $X = F^{-1}(Y)$, while, of course, the identity of $F = F^{-1}$ is not obligatory;[2]

d) the connection $Y = F(X)$ says nothing as to succession or simultaneity in time;

e) the connection $Y = F(X)$ has not the force of effective necessity;

f) the connection $Y = F(X)$ is transitive, i.e. if $Y = F(X)$ and $X = G(V)$, then $V = H(Y)$;

g) the connection $Y = F(X)$ expresses only partly the causal connection $X \rightarrow Y$ and even less completely the interaction $X \leftrightarrow Y$;

2. From a comparison of these types of regularities it is obvious that the time sequence in which all the processes in reality take place, and which possesses the substantial property of one-sided direction (from the past to the present and so on to the future), constitutes the obligatory characteristic of causality, conditioning the asymmetry of causal connection; but we do not identify causality with mere sequence in time. We must recognize that if two processes X and Y are objectively connected (materially), i.e. if $X \leftrightarrow Y$, while X precedes Y, i.e. $X \circ\!\!\rightarrow Y$, it follows that causal connection, i.e. $X \rightarrow Y$, exists between them.

3. From a further comparison of these types of regularities we see that efficacy, or activity, is not only an essential, but also a specific characteristic of causal connection, which distinguishes it from mere sequence in time, since the latter is considered detached from the processes that take place in it.

4. By efficacy we should not understand something extraneous, something applied to matter. One should not conceive it as something emanating from matter, like the Newtonian idea of force necessary to put inert matter into motion. Matter itself is both an active and passive principle. The active principle is connected with the time-form of existence of matter, the passive

[2] This identity takes place only for symmetrical functions, for instance, if

$$y = \frac{x + a}{bx - 1},$$

then

$$x = \frac{y + a}{by - 1}.$$

with the space-form. Of course the concepts of efficacy as well as of force, at the very beginning, acquired an anthropomorphic character, connecting themselves with muscular sensation as an expression of the movement of our body and of its effects on the outside world. The concept of efficacy of activity should be purged of these anthropomorphic features which include both mechanistic and finalistic factors.

5. Causal connection is not included in functional dependence as an incidental case, for the absence of symmetry and the presence of efficacy and conjunction in time, are the tokens which characterize causal connection as against functional dependence. That is why theoretical physics, in studying material processes with the aid of functional dependence, is constrained to introduce special conditions, giving it an interpretation different from mathematics.

6. First of all, time (t) as a variable is added to functional dependence.[3] This t constitutes an exceptional independent variable—from the standpoint of physics $\frac{dt}{dx}$ is senseless—and is substantially the sole independent variable in physics. All quantities which characterize the condition of the physical system are considered as manifold functions of time $g_k(t)$. Thereby, not only sequence in time is introduced, but also asymmetry, although of a somewhat different character from the asymmetry of causal connection. The independent variable, t, does not act directly upon the dependent X. Instead of this there is only a possibility of free choice of the specific value of t_0 which determines the specific value of X_0. At the same time, a continuous, uniform change of t is presupposed.

7. However, even in this changed form, functional dependence of theoretical physics differs sharply from causal connection. It does not contain—at least explicitly—the factor of efficacy. Formally it rests on the view-point of description and not of explanation, and only the interpretation of symbols introduces causal conception and banishes phenomenalism. Further, in the functional dependence of theoretical physics the one-sided direc-

[3] Cf. also G. Windred, "The History of Mathematical Time," in "Isis," Nos. 55, 58 (1933).

tion that is characteristic of causal connection is absent. This occurs because the variable t, like all variables in mathematics and geometry, is bi-directional, whereas real physical time is uni-directional. Thus, in the four dimensional world of the theory of relativity (x, y, z, t) the coordinate of time differs from the space coordinate not only in being measured with the imaginary unit $\sqrt{-1}$, but also by the fact that ds must always be greater than zero.

8. Physical idealism, notably Machism, deduces the uni-directionality from the irreversibility of macroprocesses, banishing the concept of "uni-directionality" of time, as well as the concept of time itself for microprocesses in favor of statistics.[4]

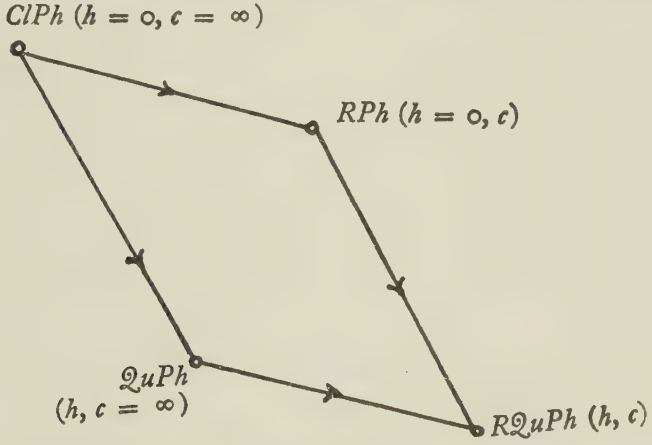
From Heisenberg's "uncertainty principle" they deduce not that we have no right to regard electrons as globules, nor that the physical quantities are indefinable, but that they are indefinite in principle. From Schroedinger's equation they deduce not the temporal limitation of our knowledge, but the "decline of determinism"[5] while determinism is identified with dynamic regularities so that statistical regularities may be made equivalent to indeterminism.

9. Thus, theoretical physics, becoming more and more transformed into branches of both geometry and "algebra," is losing both matter and determinism, while a very important rôle in that process is played by the concept of time that is detached from matter, that is "*dephysicized.*" Hence it follows that the efforts to unify the bifurcated physics in a purely formal way are

[4] Extremely characteristic are the arguments of E. Zilsel in an article "Ueber die Symmetrie der Kausalität und die Einsinnigkeit der Zeit." "Naturwissenschaften," No. 12, 1927, where the proposition of "unidirectial time being included in the second principle of thermodynamics" is coordinated with arguments springing from it against the materialistic conception of history. Cf. also H. Reichenbach, "Axiomatik des relativistischen Raum und Zeitlehre," 1924; R. Carnap, "Die Abhängigkeit der Eigenschaften des Raumes von denen der Zeit," 1925; Zilsel is, however, compelled to admit the existence of a connection between the unidirectionality of time and the difference between mass (which is his terminology for "matter") and field and consequently, the impossibility of reducing the unidirectionality of time to the second law; nevertheless he is reluctant to carry out this argument to the end.

[5] Cf., for instance, A. Eddington, in "The Decline of Determinism," "Nature" of February 13, 1932, makes the following deduction: "So far as the physical universe is concerned, determinism appears to explain nothing."

bound to end in failure; for it is a case of a revision of concepts, and above all, of the *concept of time*, as this was quite clearly stated by Schrödinger.[6] Therefore, the unified physical theory of matter, relativist quantum physics, or what is the same, quantum relativist physics, cannot be obtained by simple inference, by the addition on a parallelogram of classical physics (*Cl Ph*), relativist physics (*R Ph*), quantum physics (*QuPh*), and relativist quantum physics (*R QuPh*), by passing on from extreme cases for $h = 0$ or for $c = \infty$ to a case with finite values of these quantities, but by a qualitatively different method which includes in the very basic concepts the guarantee of a solution



of existing contradictions which, by the way, are not solved even in non-relativist quantum mechanics.

10. The way for the creation of a relativist quantum physics should include first of all, a correct interpretation of the quantum of action *h*,—the elementary action of matter—which represents the product of energy by time. Since it has been demonstrated

[6] E. Schrödinger, "Spezielle Relativitätstheorie und Quantenphysik," Sitzungsberichte der Preuss. Akademie der Wiss., no. 12, 1931, p. 328, where he says: "*Durch diese Sonderstellung der Zeit* erweist sich die Quantenmechanik in ihrer gegenwärtigen Gestalt und vor allem in ihrer gegenwärtigen *Interpretation* als durch und durch unrelativistisch. Das wird nicht behoben, wenn man blos durch Adaptierung des Formelapparates eine rein äusserliche Gleichstellung das heisst formale Invarianz gegen Lorentztransformation herbeiführt."

that this action in respect to space extends to the entire universe, its discontinuous character has to be attributed to the factor of time, allowing for elementary portions of time, for its *quantification*. This notion was broached time and again in scientific literature, not so much by speculative physicists as by theoreticians working on the basis of experiment. Thus, J. J. Thompson in his work "The Intermittence of Electric Force,"[7] writes:

"We shall suppose that the electric field has a structure in time and instead of the electric force being a continuous effect, consisting of a continuous flow of momentum into the body under its action, it is really discontinuous and consists of finite increments of momentum separated by finite intervals of time. On this view, when a body at a place P is acted on by a force, we mean that there is at time δt some chance of its receiving an increment of momentum, let this chance be $\frac{\delta t}{\mathcal{T}}$, $\mathcal{T}$ will be the average time between the increments." For the element of time Thompson gets a quantity on the order of 10^{-21} seconds. Some suggestions in the same direction were expressed also by other authors, as for instance by H. Latzin in a nebulous and mystified form in his essay "Quantentheorie und Relativität,"[8] saying that the atomic world represents a leap form fluctuation of the reality, and demanding, though very indefinitely, that there be "a qualitative distinction between time and space."

11. The question of discontinuity of time in connection with the problems of recent physics was first put by Robert Lévi in the article "L'atome dans la théorie de l'action universelle et discontinue," C.r. *183*, 1026, 1626, which computed for an atom of time the value $4{,}5 \times 10^{-24}$ *sec.* The same question was treated by G. J. Pokrowsky (Moscow) in a number of articles contributed to the "Zeitschrift für Physik," beginning in 1928, namely in Vol. 51, Nos. 9–10 (1928), "Zur Frage nach einer oberen Grenze für die Energiedichte," "Zur Frage nach der Struktur der Zeit," and further in Vol. 65, Nos. 1–2 (1930), "Zur Dirac'schen Theorie

[7] Proc. of the Royal Soc. of Edinburgh, XLVI, p. 90.
[8] "Naturwissenschaften," No. 87, 1927.

von Protonen und Elektronen." The same set of notions is worked out by G. Beck (Zürich) in the same Journal, Vol. 53, Nos. 9–10 (1929), "Die zeitliche Quantelung der Bewegung."

Pokrovsky first starts from the assumption that the density of energy has the same upper limit as the density of the mass in the atomic nucleus and hence deduces a maximum density of energy of the order of 10^{14}, a maximum possible temperature of the order of 10^{13}, and a maximum frequency of radiation of the order of 10^{23}. He goes on to point out the possibility of "experimental verification" of the existence of atoms of time of the order of 4.3×10^{-24} *sec.* by means of comparison with the period of the shortest vibrations of penetrating radiation, and he alludes to observations by Millikan and Cameron in 1926 which confirm this supposition.

Beck tries to motivate systematically this assumption by presuming that at the basis of any motion lies the velocity of light, so that the motions of lesser velocities are lapses into states of rest lasting from one to several elements of time. From the relation of $h\nu_0 = Mc^2$ he obtains for the atom of time, which he designates as a "chron":

$$1\varkappa = \frac{1}{\nu_0} = \frac{6.55 \times 10^{-27}}{1.66 \times 10^{-24} \times 9 \times 10^{20}} = 4.37 \times 10^{-24} \text{ sec.}$$

Hence Beck arrives at the existence of an atom of the extension of "odon" that is equal to 1.31×10^{-13} *cm.*, and he verifies the obtained results by substituting them in the formulae of dimensions of the basic electromagnetic quantities, and comes to the conclusion that, whereas to the mass of an electron there corresponds a frequency of one vibration per 7.0×10^7 chrons, to a heavy mass there corresponds a frequency of one vibration to each chron. This he takes to be the explanation for the inaccessibility of gravitational waves to observation, and he finally comes to the conclusion that the density of energy in space is a constant, that no transposition of energy takes place.

Referring to Beck's *quantification* of time, Pokrovsky in his

work of 1930, on the basis of the theory of Dirac, from the equation

$$\epsilon = \frac{m_0 c^2}{\pm \sqrt{1 - \frac{v^2}{c^2}}}$$

deduces the existence of an upper limit of the energy of the quantum, and defines the maximum possible frequency of radiation

$$\nu = \frac{(m_0 + m_0)c^2}{h}$$

in which m_0 and M_0 are rest masses of electron and proton, whence for one chron the value is 4.3×10^{-2} *sec.* The least possible time between two leaps of an electron is

$$\theta = \frac{\lambda_0}{c},$$

where λ_0, the diameter of the electron, coincides with this quantity; $\frac{v}{c}$ is the probability of the leap, where v is the velocity of the proton.

Further application is given to the theory by E. Regener in "Messungen uber das kurzwellige Ende der durchdringenden Hohenstrahlung," "Naturwissenschaften," 11 (1929), and in the domain of regularities of astrophysics by W. Anderson in "Zur Theorie von G. J. Pokrowski über die obere Grenze für die Masse eines Sternes," Ztschr. f. Phys., *53*, 547 (1929), and "Die Theorie von G. J. Pokrowski und die Kontraktaticnsenergie der Sterne," Ztschr. f. Phys., *55*, 386 (1929), and also in thermodynamics by S. Suzuki in "Die obere Grenze der Energiedichte und einige Thermodynamische Schlussfolgerungen," Phys. Ztschr., *13*, 618 (1930).

In an analogous way F. O. Wollaston in "The Nature of Time," Nature, *31*, I (1931), obtained 8.12×10^{-21} *sec.* as the value of a time quantum from the expression h/mc^2 by substituting for m the mass of the electron (instead of the proton).

Of decisive value for this theory will be the results of future measurements of the wave length of the hardest components of cosmic radiation.

12. As seen from the brief analysis given here, these works either, as in the case of Pokrovsky, fail to develop beyond the starting point or, as in the case of Beck, drop again into the "equalization" of time and space on the ground of their common discontinuity. In scientific literature there are efforts at quantification of space which give some positive partial results, yet they are incapable of giving a generalized unified theory. Thus, V. Ambarzumian and D. Ivanenko in the article "Zur Frage nach Vermeidung der unendlichen Selbstruckwirkung des Elektrons"[9] attempt to overcome the contradiction encountered in quantum electrodynamics of the formal appearance of the infinite reaction of the electron upon itself, by introducing, instead of differential equations, equations in finite differences, considering three-dimensional space as an integral cubic point-cell, and arriving at the admission of minimum wave-length.

$$\Delta x = \frac{he^2}{mc^2},$$

of maximum frequency

$$\nu = \frac{M+m}{h} c^2 = \frac{1}{\Delta t},$$

and of elementary time interval

$$\Delta t = \frac{1}{c} \Delta x.$$

Another contradiction of quantum electrodynamics, the formal appearance of infinitely great energy in the origin of coordinates of empty space, is not eliminated. In another work, "Die Beobachtbarkeit in der Diracschen Theorie,"[10] D. Ivanenko relying upon the theory of "individual errors" propounded by Landau and Peierls,[11] deduces from Schrödinger's vibrating

[9] Ztschr. f. Phys., *64*, #7–8, 563 (1930).
[10] Ztschr. f. Ph. 72, 9–10, 1931, S.621.
[11] Ztschr. f. Ph. 69, 6–6, 1931.

motion the least determinable distance

$$\lambda \sim \frac{h}{mc}$$

and hence the least interval of time. In this connection it is necessary to note the work of A. Markov "Über die Ableitbarkeit der Weltmetrik aus der 'Früher als'-Beziehung,"[12] in which the author shows that by admitting in atomic structure the relation of "time precedence" $X \circ\!\!\rightarrow Y$, it is possible in a formal axiomatic way to deduce from it the metrics of space-time, while these metrics for "great" intervals will coincide with the "ordinary" one.

13. In distinction from these assumptions, we propose to proceed from the discontinuity of time and the continuity of space; the four-dimensional space-time interval will then be continuous on three axes and will have a cellular structure on one axis. Hence it follows that Lorentz's invariant of transformation: $dx^{12} + dy^{12} + dz^{12} - c^2dt^{12} = dx^2 + dy^2 + dz^2 - c^2dt^2$ will have to assume such a form that instead of differentials of time it would include finite increments of time, integral multiples of 1 chron. This we will achieve, bearing in mind that the principle of invariance of the velocity of light in a vacuum is deduced from the extreme abstraction—the point, non-material source of light. From this will result a different form of the equations of the electron theory. Thus for instance, in

$$\frac{\partial F^{ik}}{\partial x_k} = s^i \qquad (i, k = 1, 2, 3, 4),$$

$$\frac{\partial F_{kl}}{\partial x_i} + \frac{\partial F_{li}}{\partial x_k} + \frac{\partial F_{ik}}{\partial x_l} = 0 \qquad (i, k, l = 1, 2, 3, 4)$$

instead of

$$\frac{\partial \mathfrak{E}_x}{\partial t}, \frac{\partial \mathfrak{E}_y}{\partial t}, \frac{\partial \mathfrak{E}_z}{\partial t}, \frac{\partial \mathfrak{H}_x}{\partial t}, \frac{\partial \mathfrak{H}_y}{\partial t}, \frac{\partial \mathfrak{H}_z}{\partial t}$$

will enter finite increments, and so forth. The "odons" obtained by Beck will then have only formal, but not physical meaning.

[12] Phys. Z. d. Sowjetunion, I, 3, 1932, S.397.

Since we shall have to deal with equations containing both differentials (or their integrals upon the continuous space interval) and finite differences, the apparatus of tensor analysis will have to be adjusted accordingly.

14. It is necessary to emphasize with all clearness that the discontinuity of time is by no means conceived by us in the sense of absolute discontinuity, the "atoms" of time, or chrons, are not presumed to be final indivisibles.[13] The essence of the introduction of discontinuous time consists in that just time, and not space, realizes in the space-time state of matter its structural character. At the present stage of our knowledge, since we investigate processes, which do not descend, for instance into the depth of the electron, the inevitable further divisibility of the chron—again assuming, however, an integral figure—is not revealed. It is further necessary to note that of the very "mechanism" of the leap from chron to chron we are as yet unable to say anything. We should warn, however, that the very idea of the discontinuous structure of time may appear to be absurd because it is unusual, although on the other hand some eminent psychophysiologists[14] on the grounds of an analysis of individual perceptions of time, did insist on the discontinuity of time, which in itself nevertheless gives as little support to this supposition as the "common sense" arguments fail to prove anything against it.

15. The way suggested here for the creation of a unified physical theory of matter, after mathematical elaboration of this theory, will be justified, only if it be possible: (a) formally to combine relativist electrodynamics and the theory of gravitation with wave mechanics, as is the case in the projective general field-theory of Schouten and Van Dantzig[15] which satisfies the

[13] Discontinuity of time, as indeed any other theory, may be splendidly made use of by idealists and theologians. Cf. for instance the views of Mutakillimûn, the Arabian mediæval philosopher of orthodox-Islam tendency, who supposed that time is discontinuous and that God at each new instant, by a special act, creates the world anew.

[14] Cf. for instance the work (Russian) of I. M. Sechenov "Reflexes of the Cerebrum" (1863) and N. E. Vvedensky "On the correlations between irritation and excitement" (1886) or Lapique's "L'excitabilité en fonction du temps" 1926.

[15] J. A. Schouten and D. Van Dantzig "Zum Unifizierungsproblem der Physik, Skizze einer generellen Feldtheorie." Koninklijke Akademie van Wettenschappen te Amsterdam, Proc., XXXV, 5, 1932.

following requirements of physics: (1) the geodesic lines represent world lines of charged particles of mass; (2) in the field of gravity the displacement of vectors is the Riemann displacement known from the ordinary theory of relativity; (3) the simplest directly obtained linear equation is identical with Dirac's equation; (4) the total point of impulse-energy (in the projective theory it takes the place of the vector of impulse-energy) constitutes along the world line a co-variant constant; (5) the variation of the simplest directly obtained invariant gives a combined equation of the field of gravity and electromagnetism; (b) to obtain a new result—a non-contradictory relativist quantum physics which will not only eliminate the contradictions in non-relativist quantum physics itself, but will also give new correlations that will be confirmed experimentally.

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Outline of a Mathematical Theory of Human Relations

BY

N. RASHEVSKY

I

IN OUR previous writings[1, 2, 3] we have outlined a mathematical theory of biological phenomena. In our systematic construction of "mathematical biology," similar in its aims to mathematical physics, we have started with the fundamental unit,—the living cell. After having established a physico-mathematical theory of the fundamental properties of the cell, we have studied the interaction of several cells. This led us into two different fields. On the one hand we studied such interactions of cells, which determine the form of cellular aggregates, constituting multicellular organisms. On the other hand we studied different types of functional interactions, which determine the reactions of the aggregate *as a whole* to different environmental changes. While the first field leads us eventually to the theory of organic form, the second brings us to the physico-mathematical theory of behavior.

As we have seen previously, even some highly complex char-

[1] Phil. of Science, *1*, 176, *1934*.
[2] Phil. of Science, *1*, 409, *1934*.
[3] Phil. of Science, *2*, 73, *1935*.

acteristics of behavior are thus amenable to physico-mathematical interpretation, and hence to mathematical treatment.[2, 3, 4]

The reaction of any organism is determined by its surroundings at a given moment as well as on the variations of the surroundings in its past. But the environment of an organism is in itself constituted of two parts: first of the inorganic environment and then of other organisms. Hence any mathematical theory of organismic reactions and behavior must of necessity include the study of interaction between various individual organisms. An approach to the mathematical study of biological interactions of organisms of different kinds, belonging to different species, has been done in the masterful works of A. J. Lotka,[5] V. Volterra[6] and others. We shall discuss here the mathematical approach to the study of interaction of organisms of the same species, and consider the more complex problem of psychological interaction, involving complex behavior. In other words, we shall attempt no less a thing than a mathematical theory of human relations. This leads us into the field of social science.

We must however *make it clear at once and emphasize it very strongly* that we do not propose here to add another "theory" to the great number of those already existing in sociology.[7] We do not propose to prove by mathematical methods the preponderant or even exclusive influence of any one particular factor, such as biological, psychological, geographic, demographic and others, in social phenomena. We do not propose at all to establish any "laws" or "relations," which may be claimed to be actually in existence. Still less do we intend to draw any merely formal analogies from physics in the manner of the so-called "social mechanics."[8]

What we intend to do, is to build up a mathematical system describing *possible* interrelations between the individuals of a group, regardless of any counterpart of such relations in reality;

[4] Jl. Gen. Psych., *5*, 207 and 368, *1931*, also July, 1935.
[5] A. J. Lotka. Elements of Physical Biology.
[6] V. Volterra. Leçons sur la théorie mathématique de la lutte pour la vie.
[7] P. Sorokin. Contemporary Sociological Theories.
[8] Haret. Méchanique sociale.

just like the various systems of geometry: Euclidian, Lobatschevsky's, Riemann's have been built up "in abstracto," regardless of any applications to the physical universe. We shall stick to the principles, which were laid down in our article on the "Foundations of Mathematical Biophysics."[1]

If questioned as to the value of such purely abstract speculations, we shall answer as before: such has been the way of development of all mathematical sciences. In order to have a mathematical theory of concrete, real phenomena, we *must* first possess the abstract structure. Einstein's discoveries of the actual geometry of the real universe would have been impossible, if Riemann's geometry had not already been given as an abstract structure.

Another point which needs emphasis, is that we are giving in this article only an *outline*. We shall neither elaborate any details nor attempt any mathematical rigor. But we shall *actually consider fundamental equations*, which are characteristic of a mathematical theory of human relations, and not merely advocate the necessity of the mathematical method.[9]

II

In the absence of any other individuals, the behavior of an individual would be determined by his immediate surroundings, his psycho-physical constitution, and his past history. We have shown previously, that a physico-chemical interpretation of such phenomena as volition and desire is possible.[10] If *the* real interpretation were actually known, those purely psychological quantities would be amenable to quantitative expressions in terms of the intensities of the corresponding physico-chemical processes in the organism, and natural units for their measurements would thus be established. Our very limited knowledge of those underlying physico-chemical processes makes such a thing still impossible *at present*. But inasmuch as we admit a physico-chemical interpretation of psychology, we should not hesitate to introduce such things as "desire" or "will" as quantities in our

[9] V. Pareto. Traité de sociologie générale.

[10] Jl. of Gen. Psych., *5*, 368, *1931*.

equations and consider them *measurable in principle*, although they may not be measurable *directly*. As a matter of fact, such an introduction of those quantities into our fundamental equations may ultimately result in the possibility of their *indirect* measurement. In developing mathematically the consequences of those equations we may find amongst various expressions thus obtained, such as connect those quantities with other, directly measurable ones. Such a procedure is not unfamiliar to physics, and in our opinion those psychologists, who declare war on anything, which is not directly measurable and invoke as a justification of this war the example of physics, are "plus royalistes que le roi."

In classical electrodynamics such fundamental things as the electric and magnetic vectors are not *directly* measurable quantities. They are measured *by definition* by the force, which is exerted on a unit of charge. And the force is also measured indirectly by measuring displacements of a dial on a scale or by observing accelerations. Of course, the *indirectness* of the measurements here is *relatively* small, but this does not affect our argument in principle. Some may quite legitimately prefer to determine the electric and magnetic vectors as quantities, that satisfy Maxwell's equations. The indirectness of their measurement becomes then still more evident. Physics has been attempting to eliminate quantities, which are not *measurable in principle*. But there is a great difference between "measurable in principle" and "measurable directly." In considering the prominent rôle of Schrödinger's Ψ-function and its generalization by Dirac, we may still question whether the complete elimination even of *quantities not measurable in principle* will be the ultimate goal of physics.

As regards the comparative value of direct and indirect measurements, witness the fact that calculations of the values of e and h (electronic charge and quantum constant) from spectroscopical data lead to much more accurate values, then direct measurements.

Inasmuch as *at present* we are dealing with purely abstract constructions the introduction of some directly not measurable quantities is justified *a fortiori*.

III

After those preliminaries let us consider the various possible forms of equations for the interaction of individuals.

Denote the intensity of some activity i by a_i. In the absence of other individuals a_i will be proportional to the desire w_i for this activity, which is in its turn determined in general by the past history of the individual. If another individual is present, he will in general influence the activity of the first one. This influence will be determined by two factors: the desire w'_i of the second individual for a_i, as well as the amount of influence, which the second individual exerts on the first. This influence may be of different nature; it may reside in physical force (which happens to be an *almost directly measurable* quantity!) or in any other sort of influence, such as moral, religious, etc. Considering first the simplest possible law of interaction, a *linear* one, we shall put the influence of the second individual upon the activity of the first one as proportional to a coefficient of influence $F' > O$ of the second individual, and to w'_i. Hence we have

$$a_i = \alpha w_i + \beta F' w'_i \tag{1}$$

α and β being constants. w_i and w'_i may be either positive or negative. If $w_i > O$, $w'_i < O$, the second individual may be said "not to like a_i," and attempts to reduce the activity of the first.

Let us now consider a large group of $\mathfrak{N}$ interacting individuals, and first suppose that there is only one sort of activity a, which they all can perform. We can characterize in this case each individual by his F and w. In general all individuals differ from each other. There will be a distribution function

$$N(F, w)\, dF\, dw \tag{2}$$

which indicates how many individuals are characterized by F and w lying in the intervals F, $F + dF$ and w, $w + dw$. The distribution function (2) satisfies the relation:

$$\int_0^\infty N(F, w)\, dF\, dw = \mathfrak{N} \tag{3}$$

Every individual (F, w) performs the activity with the intensity $a(F, w)$, which is determined by his own w and by the influence of all others. Accordingly equ. (1) becomes now:

$$a(F, w) = \alpha w + \beta \int_0^\infty N(F', w')\, F' w'\, dF'\, dw' \tag{4}$$

In the most general case we may consider the function $N(F, w)$ as possessing a finite number of discontinuities. In general we shall have an integral and an ordinary sum, the latter extending over single individuals, which due to their very large F, may exert a large influence on the activities of others.

Equation (4) determines the activity of each individual in terms of the influence on him of the rest of the community.

We may consider a more general case of interaction, which is closer to real conditions. We may assume the influence of an individual (F', w') on (F, w), as depending on the difference $F' - F$. If there is an influence only when $F' > F$, then we have

$$a(F, w) = \alpha w + \beta \int_F^\infty N(F', w')(F' - F)\, w'\, dF'\, dw' \tag{5}$$

In the still more general case, when there is an influence even for $F' < F$ we find:

$$\begin{aligned} a(F, w) &= \alpha w + \beta \int_0^\infty N(F', w') \mid F' - F \mid w'\, dF'\, dw' \\ &= \alpha w - \beta \int_0^F N(F', w')(F - F')\, w'\, dF'\, dw \\ &\quad + \beta \int_F^\infty N(F', w')(F' - F)\, w'\, dF'\, dw' \end{aligned} \tag{6}$$

A still more general case to be considered next is that the activity of an individual is not only affected by his own desire for it, and the desire of others, but that it is also affected by the activities of the others. An individual may *imitate* another one, and if this other individual engages in an activity a', then the first individual will produce a the stronger the stronger a'. We have:

$$a = \alpha w + \beta \mid F' - F \mid w' + Ia' \tag{7}$$

where I may be called the *coefficient of imitation.* It is to be considered as a third characteristic of an individual, together with F and w. We now have for the whole group of $\mathfrak{N}$ individuals:

$$a(F, I, w) = \alpha w + \beta \int_0^\infty N(F', I', w') \mid F' - F \mid w' \, dF' \, dI' \, dw' + \gamma I \int_0^\infty N(F', I', w') \, a(F', I', w') \, dF' \, dI' \, dw' \tag{8}$$

γ being a constant.

The individual may imitate the activity of another one in proportion to the influence F' of that other. We then have:

$$a(F, I, w) = \alpha w + \beta \int_0^\infty N(F', I', w') \mid F' - F \mid w' \, dF' \, dI' \, dw' + \gamma I \int_0^\infty N(F', I', w') \, F' \, a(F', I', w') \, dF' \, dI' \, dw' \tag{9}$$

For a given $N(F, I, w)$ the first integrals of the right hand sides of (8) and (9) are known constants, and (8) and (9) are integral equations for the determination of $a(F, I, w)$, with respective degenerate kernels:

$$K(F, I, w, F', I', w') = IN(F', I', w') \tag{10}$$

and

$$K(F, I, w, F', I', w') = IF' \, N(F', I', w')$$

Equations of one of the above discussed type will hold in a more general case of several activities a_i. As an illustration we may consider the case of two different activities under the following assumptions: the activity a_1 of an individual is suppressed or opposed by the other ($w'_1 < O$), unless the first does also a different activity a_2, which the second likes ($w'_2 > O$) and for the sake of which he is willing to let the first do some a_1. We then have:

$$\left.\begin{aligned} a_1 &= \alpha w_1 + \beta(1 - a_2) \int \mid F' - F \mid N w'_1 \, d\tau \\ a_2 &= \alpha_1 w_2 + \beta_1 a_1 \int \mid F' - F \mid N w'_2 \, d\tau \end{aligned}\right\} \tag{11}$$

where the explicit indication of the variables is omitted for brev-

ity. N is now a function of F, I, w_1 and w_2 and $d\tau$ stands for $dF'\,dI'$, $dw'_1\,dw'_2$.

Similar expressions may be obtained for more general cases.

We may consider a case in which there is a relation between F, I and w for every individual, due to his biophysical constitution. Then we can reduce the number of variables in the above equations. If for instance: $I = f(F)$ and $w = f_1(F)$ then F remains as the only independent variable.

In general F will be composed of various components:

$$F = F_1 + F_2 + \cdots + F_i \tag{12}$$

such as physical force, moral persuasion, religious influence, intellect, etc. We may consider a great variety of cases, where the influence of the various factors is different. Instead of (1) we may have:

$$a = \alpha w_i + \beta \sum_i \eta_i (F'_i - F_i)\, w' \tag{13}$$

with the corresponding equation for the whole group:

$$a = \alpha w + \beta \int N \sum_i \eta_i (F' - F)\, w'\, dF'\, dw' \tag{14}$$

The numbers η_i show the relative importances of the various components F_i of the total F.

We may also consider the more complex case, that w consists of two components: one component u represents the desire of an individual to do a himself; the other component v represents the desire of having others do a. We may have $u < O$, $v > O$, which means that the individual likes to see the others do a, but does not like to do it himself. We then have:

$$a = \alpha u + \beta v + \gamma \int (F' - F)\, N(F', u', v')v'\, dF'\, du'\, dv' \tag{15}$$

and similar equation for more complex cases.

Any of the above equations may be made the starting point of an abstract theory of human relations. In other papers we shall make a systematic study of all the cases outlined here. Now we must restrict ourselves only to some general considerations.

Equations (4), (5), (6), (8), (9), (11), (14) and (15) all have this in common, that the activity of every individual is dependent on the psycho-physical constant of all other individuals. In equations (8) and (9) it also depends on the actual *activities* of those other individuals. The activity of every individual depends furthermore on N, which characterizes the biological constitution of the whole social group. In further developments we may study the more complex cases of interaction of several groups, characterized by different N. In this way we may study mathematically any possible influence of racial and ethnographic factors. For a given $N(F, w)$ our equations determine completely to what degree the activity of any individual is influenced by others. In particular we may investigate, under what conditions will the activity of a minor group remain very little influenced but will itself influence the activities of the remaining major group. For the case of (4) this will happen for instance when $\int_0^\infty N(F, w)\, Fdw$ will increase sufficiently strongly with F. In general the larger F, the more independent the activity of an individual. But in the general case represented by (14), this is not necessarily so. A large total F does not yet mean a large independence for any activity. The influence of the various components F_i must be studied.

Without going here into details, which require a more thorough mathematical study, we see, that we can describe what would be ordinarily called social structure and relations in quantitative mathematical terms. Considering some of the activities as more directly measurable by statistical methods, we shall seek in our study of the consequences of various equations, such relations, which express in terms of these measurable quantities the others, such as F, I, w.

It may not seem at first to be much worth the trouble to find mathematically that individuals with a large F have their activity very little influenced by those with a small F. It is fairly obvious, one would say. But the value of the mathematical investigation lies not at all in the statement of this qualitative relation. That can be done without any mathematics. The value of the above equations lies in the circumstances, that they give definite analyti-

cal relations between F and a, for instance. And, as we have already emphasized, it is only through such relations that we find the possibility of observing and measuring some of the more elusive, directly not accessible, quantities.

IV

Let us now pass to the consideration of a different type of phenomena in human aggregates. Let us study the case, where the individuals tend to associate in groups or classes. In general the tendency between two individuals to associate will be the greater, the more similar the individuals. We may however consider also cases of association between individuals with largely varying characteristics. For the present we shall restrict ourselves to the former case.

The simplest possibility is that two individuals associate only when the absolute value of the difference of their characteristics is less than a certain critical amount. For the case of one variable F, we shall have as condition of association:

$$(F' - F)^2 < \Delta^2; \qquad (F' - F)^2 - \Delta^2 < 0 \tag{16}$$

Consider the formation of an association of individuals, which will include those having the highest F, which we denote by F_m. How far down the scale of F's will individuals still be included in that association? The answer is given by the requirement that the sum total of $(F' - F)^2 - \Delta^2$ for the whole group would remain negative. If F_x is the lower limit of F, still admissible into the association, then:

$$\int_{F_m}^{F_x} \int_{F_m}^{F_x} [(F' - F)^2 - \Delta^2] \, N(F) \, N(F') \, dF \, dF' < 0 \tag{17}$$

The left hand side of (17) is a function of F_x, $P(F_x)$; F_x then is the root of the equation:

$$P(F_x) = 0 \tag{18}$$

Individuals with $F < F_x$ are not admitted into the "social class" of the larger F's. Similar relation can be established for several variables, characterizing an individual.

An expression built similarly to (17), but when the integration is carried out between the limits F_x and $F_{x'}$, determines the size of the next "class," in which the maximum value of F is F_x, and the minimum $F_{x'} \cdot N(F)$ and Δ determine the total number of "classes," which naturally form in a given society. If $N(F)$ is known, we can calculate Δ from the (usually small) number of "classes."

Other criteria for association into social classes can also be studied. Similarly to (13) we may consider as criterion of association:

$$\Sigma\zeta_i(F_i - F'_i)^2 < \Delta^2 \tag{19}$$

Finally we may take as criterion not the psychobiological difference but differences in activities. We then have:

$$(a' - a)^2 < \Delta^2 \tag{20}$$

a being determined as a function of F and other variables by the fundamental equation chosen to represent a given social interaction. We can determine the number of individuals $N^*(a)da$, which have an a between a and $a + da$. Then we find

$$\int_{a_m}^{a_x}\int_{a_m}^{a_x} [(a' - a)^2 - \Delta^2]\, N^*(a)\, N^*(a')\, dada' = 0 \tag{21}$$

as determining the size of the class.

V

Thus far we have considered the interaction of the individuals as being independent on their distribution in space. The next step is to consider the more general case, where the location of an individual affects his interaction with others, as well as his activity. In this way we can take into account and study mathematically the influence of various geographic factors.

First of all the desire for a certain activity may depend on the location of the individual. So that now, considering only F and w as independent variables, characteristic of the individual, we have $w = w(x, y)$, x and y being the geographic coördinates. The interaction of two individuals will also in general be a func-

tion of their mutual distance $f(x'-x,\ y'-y)$. As a rule it will decrease with increasing distance. We must also consider a more general distribution function $N(F, w, x, y)$, which not only gives the distribution function with respect to F and w, but also indicates the number of individuals located in the area $(x, x + dx)$ $(y, y + dy)$.

If the influence of the locality is *not selective* for individuals of a certain characteristic, we shall have:

$$N(F, w, x, y) = N_0(F, w)\, N^*(x, y) \tag{22}$$

But in general this need not be the case. As the fundamental equation for the structure of society, we shall now consider:

$$\begin{aligned} a(F, w, x, y) &= \alpha w(x, y) \\ &+ \beta \int (F' - F) f(x' - x, y' - y) N(F', w', x', y')\, dF' dw' dx' dy' \end{aligned} \tag{23}$$

or similar but more general equations.

The combination of the study of the "geographic" factor with the study of formation of "classes" leads to some very interesting results and formulae. We shall briefly indicate here one case *only as an illustration.*

Consider a case, when the whole group of $\mathfrak{N}$ individuals divides naturally, according to (18), in only *two* classes. If for such a division the inequality

$$\int_0^{F_x} N(F) F dF < \int_{F_x}^{F_m} N(F) F dF \tag{24}$$

then the total influence of the class (F_x, F_m), which we shall call "first class" is larger, than the total influence of the class (O, F_x), which we shall call "second class." We shall mark all quantities, referring to individuals of the first class with $(')$, and all quantities, referring to individuals of the second class with $('')$. The expression

$$\begin{aligned} \int_{F'-F_x}^{F'-F_m} \int_{F''=0}^{F''-F_x} \int_S \int_S (F' - F'') N'(F', x', y',) N''(F'', x'', y'') \\ \times f(x' - x'', y' - y'') dF' dF'' dx' dx'' dy' dy'' \end{aligned} \tag{25}$$

in which the integration with respect to x', y', x'' and y'' is taken over the whole region, occupied by the total group, can be considered as a measure of the extent, to which the second class is influenced or "controlled" by the first. If $N''(F', x'', y'')$ is given, that is, if the distribution of the population of the second class is known, we may ask what should be $N'(F', x', y')$ in order to make (25) a maximum. In other words, what should be the distribution of the population of the first class in order to achieve the best control of the second?

A very elementary example of such a problem may be given by considering the special case, where two individuals interact only if their distance is below a critical value r_0, and where the distribution of individuals of the second class in space is uniform with the density of q individuals per unit area. Then any group of individuals of the first class, if it is located in a very small region, can control only $\pi r_0^2 q$ individuals of the second class. But when the first class subdivides in two groups, separated by a distance $r > r_0$, then the number of controlled individuals doubles. The maximum is reached for a subdivision of the first class into

$$n = \frac{S}{\pi r_0^2} \tag{26}$$

separate groups, where S is the total area occupied by the population.

Inasmuch as $f(x' - x, y' - y)$ depends on the technical and engineering facilities at the command of the individuals, the considerations of this section indicate also the way of taking those into account. It may happen that originally a "centralized" first class will correspond to an optimum. If later on S grows, it may require a splitting into a number of groups, for better control. Later on, with increased technical means (increasing r_0) the number of groups n may again decrease, a "centralization" taking place.

VI

We have considered the influence of the past history of the individual as contained implicitly in w. We shall now consider

the possible influence of past history of the whole society on its present state.

For sake of definiteness and brevity, we shall consider here only one particular case. Let originally the structure of the group be determined by one of the equations of section III, and let it again be divided in two classes, on the basis of (18). Let us now consider the development of the society for several generations, under the assumption, that the progeny of the first class associate only with the progeny of the same class. That is, instead of the association by actual similarity, we shall have an association by the *similarity of the past generations*. For simplicity let us consider every individual as characterized only by one variable F.

Let us consider separately the distribution function for each class: $N_1(F)$ and $N_2(F)$. Originally we have $N_1(F) + N_2(F) = N(F)$. But, as we shall presently see, due to the changes of those functions with time, this relation will not hold in general for later times.

The progeny of an individual F^* will not all possess the same characteristic F^*. It will be distributed according to some distribution function $p(F^*, F)$ with a maximum for $F = F^*$. $p(F^*, F)$ represents the fraction of individuals born of a parent F^*, and possessing the characteristic F. By this definition $p(F^*, F)$ is not the same as $p(F, F^*)$. The shape of $p(F^*, F)$ can in principle be determined from genetic considerations. We shall leave it at present undetermined. Let the birth rate per individual F, be $n(F)$, so that $n(F)\ N_2(F)$ individuals are born per unit time from parents F, of the second class.
We then have:

$$\int_0^{F_x} p(F, F^*)dF^* = n(F)\ N_2(F, t)dF \tag{27}$$

We put now $N_2(F, t)$ to indicate, that due to the scattering, represented by p, the shape of N changes with time.

The total number of individuals born per unit time from parents F and having themselves $F = F^*$ is equal to

$$n(F)\ N_2(F, t)\ p(F, F^*) \tag{28}$$

and the total number of individuals with characteristic F, born per unit time of any parents of the second class:

$$\int_0^{F_x} n(F^*)N_2(F^*, t)\, p(F, F^*)\, dF^* \tag{29}$$

If $m(F)$ is the death rate, then the total change of $N(F, t)$ is given by

$$\delta N_2(F, t) = \int_0^{F_m} n(F^*)\, N_2(F^*, t)\, p(F, F^*)\, dF^* - m(F) \tag{30}$$

which is a functional equation, determining $N(F, t)$ for any t.[11]

According to our hypothesis, the progeny of all individuals of the second class continue to belong to the second class. All those from the first—to the first. Per unit time the increase in the number of individuals of the second class, which have $F > F_x$ is given by

$$\int_{F_x}^{F_m} \delta N_2(F, t) = \int_{F_x}^{F_m} dF \int_0^{F_x} n(F^*)\, N_2(F^*, t)\, p(F, F^*)dF^* - \int_{F_x}^{F_m} m(F)dF = \varphi_2(F_x, t) \tag{31}$$

A similar expression is found for the increase $\varphi_1\ (F_x, t)$ in the number of individuals of the first class with $F < F_x$. We have taken (24) as the condition of the possibility for the first class to control the second. Due to the change, represented by (31), the inequality (24) will finally cease to hold, and the social structure will become unstable, the influence of the first class having been weakened, that of the second class increased. A rearrangement is bound to occur. At what time t this instability will occur, is determined by the equation (31) together with a similar one for the first class, combined with (24). Explicit calculations for various assumptions about $n(F)$, $m(F_1)$ and the initial $N(F)$ will be given elsewhere.

We may consider the stability of a social structure from a different point of view. Whereas originally we had $a = a(F)$, we now find, that individuals with the same F may have different

[11] Paul Levi. Leçons d'analyse fonctionnelle.
V. Volterra. Fonctions des lignes.

a's, because of their belonging to different classes. To a given value of F now corresponds a multitude of values of a, which will be determined by a distribution function

$$u(F, a)\, da \tag{32}$$

giving the number of individuals with the characteristic F having an a between a and $a + da$. We have:

$$\int_0^{a_m} u\, da = N(F, t) \tag{33}$$

u itself is determined by (31) and is a function of t also. We find that now the distribution of a is no more corresponding to the "natural" distribution, given by one of the equations of section III, when a was determined by F only. Let $a(F)$ be that "natural" distribution. Then the *total* deviation of the social structure at the moment t from the "natural" one, prevailing at $t = 0$, is given by:

$$\int_0^{F_m} dF \int_0^{a_m} u(F, a)[a(F) - a]^2\, da = \psi(t) \tag{34}$$

We may consider as criterion of stability, that this total deviation should not exceed a given value h, in other words $\psi(t) \gtrless h$. The moment t at which instability occurs is given by the root of the equation $\psi(t) = h$.

A still different criterion for the stability of a social structure may be considered. In the absence of other individuals, the activity a of an individual is determined by w only, being given by $a(F) = \alpha w(F)$. Due to the interaction with other individuals, $a(F)$ does differ from $\alpha w(F)$. We may postulate, that the sum total for the community of the differences $[\alpha(F) - \alpha w(F)]^2$ should not exceed a threshold h. $\alpha(F)$ being given by one of our fundamental equations, we have another integral condition for stability, which depends on $N(F, t)$ and therefore changes with time.

In ordinary language we may state this criterion by saying, that society becomes unstable when the differences between the natural desires of the individuals and the actual fulfillment of those desires becomes excessive.[12]

[12] Cf. P. Sorokin. Sociology of Revolution.

We may also consider an intermediate case, where any individual $F > F_x$, born in the second class, can eventually pass into the first, but a certain resistance is offered to such a passage. This resistance will be in general a function of $F - F_x$. The time τ_{21} which it takes for an individual to pass from the second class into the first is also a function of $F - F_x$, which we express by writing $\tau_{21}(F - F_x)$. Its inverse can be considered as a measure of the corresponding "social mobility."[13] The total number of individuals, passing from the second class into the first per unit time is given by:

$$\int_{F_x}^{F_m} \frac{N_2(F, t)}{\tau(F - F_x)} dF = M_{21}(F_x, t) \tag{35}$$

With a similar expression for M_{12}, expressing the total number of individuals born in the first class with $F < F_x$, passing per unit time into the second class. If the above discussed instability should never occur in the course of history of a society, we must have:

$$\left.\begin{aligned} \varphi_{21}(F_x, t) &\leqq M_{21}(F_x, t) \\ \varphi_{12}(F_x, t) &\leqq M_{12}(F_x, t) \end{aligned}\right\} \tag{36}$$

φ being defined by equation (31). For then the "social mobility" will always level out the deviations from the "natural distribution."

Besides the above-discussed sort of instability, a different kind may also happen. We have seen in section III, that the structure of the social group may be characterized by an integral equation (Equ. 8 and 9). If, as will be the simplest case, those equations are linear, as in (8) and (9), they will possess only one solution. We must however in general consider the possibility of non-linear equations also. In that case it is possible that there will be two or more solutions. The situation is not without formal analogy to physico-chemical systems with several equilibria.[14] And sudden transitions from one state into the other one will have to be considered.

[13] P. Sorokin. Social Mobility.

[14] N. Rashevsky. ZS. f. Physik., *53*, 102, 1929.

VII

The question now arises, as to how to describe mathematically those transitory states, caused by one or another of the above discussed factors? In other words, when for instance a state in which $\psi(t) > h$ (equ. 34) is reached, how will the transition towards the "natural" state for which $\psi = O$, take place?

We must introduce more general equations, covering the "dynamical" cases also. They must be such, that the above discussed equations of section III would turn out as the solutions of the more general ones for "stationary states." The simplest case would be for instance:

$$\frac{da(F,I,w,t)}{dt} = -A\Big[a(F,I,w,t) - \alpha w - \beta\int_0^\infty N(F',I',w')\,|F'-F|\,w'dF'dI'dw' - \gamma I\int_0^\infty N(F',I',w')F'a'(F',I',w')dF'dI'dw'\Big] \tag{37}$$

A being a constant.

This leads to (9) as a stationary solution, for which $\frac{da}{dt} = O$. The minus sign is necessary in order that the stationary state, described by (9), would be stable. Similar expressions can be established for other cases discussed in section III, and thus the variations of the social structure, which occur during transitions from less stable to more stable states can be studied.

In general there are three possibilities which may occur. Either the stationary state is reached asymptotically, $a(F, I, w, t)$ being a monotonous function of t; or $a(F, I, w, t)$ may be oscillating with a decreasing amplitude, or finally it may oscillate around the stationary value with a finite amplitude, without a tendency to approach the stationary state asymptotically.

We must restrict ourselves at present to those sketchy remarks.

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The Techniques, Basic Concepts, and Preconceptions of Science and Their Relation to Social Study

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I. INTRODUCTORY

THE necessity for a clear understanding of the dual character of scientific method (verified observation on the one hand and theoretical analysis on the other) and of its applicability in social study as in the physical and biological sciences, can hardly receive too much emphasis at the present stage of development.[1] Such an understanding, however, merely provides the proper beginning or orientation in the organization of any scientific discipline. That which is a common element in all scientific procedure can hardly serve to differentiate one science from another.[2]

The precise differentiation, as exhibited in the history of the

[1] *Cf.*, Joseph Mayer, "Toward a Science of Society," *The American Journal of Sociology*, September, 1933, pp. 159–179; also "Scientific Method and Social Science," *Philosophy of Science*, July, 1934, pp. 338–350.

[2] Karl Pearson's statement that "the unity of all science consists alone in its method" is therefore quite superficial. That the unity of science consists also in a genetic and structural relationship between its various branches is, in addition, not mentioned. *Cf.*, "The Grammar of Science," 3rd edition, London, 1911, Part I, p. 12; also, A. Wolf, "Essentials of Scientific Methods," 2nd edition, London, 1928, pp. 15–16.

sciences, is seen to be bound up with several other important considerations pertaining to the given field of knowledge it is sought to organize scientifically: First, the matter of ascertaining the relationship of the given unorganized field to the sciences which are already established or are in process of being established; second, the matter of discovering its basic concepts; third, the matter of clearing away long-standing misconceptions which hamper scientific progress in the given field; fourth, the matter of utilizing and developing tools and techniques provided by sciences already organized. General scientific method of theoretical analysis and verified observation is applied in these four particular directions as any specific field of knowledge takes on the character of a science. Out of this concrete application develop the definite specialized procedures and categories of one science as distinguished from those of another.

These considerations seem particularly pertinent to social study at the present time, and a brief résumé of what the history of the physical and biological sciences reveals with respect to them should be helpful to an understanding of important methodological questions with which economics and sociology are now faced.

Methodological considerations exhibited in the development of the physical and biological sciences

Certain 'genetic' and 'structural' relationships between the recognized sciences have been outlined in some detail by the present writer in other connections.[3] There is a definite dependence and order in the establishment of their basic concepts:—those of mathematics came first, in ancient Greek times; then came those of mathematical astronomy, after 1600; the elucidation of these variables was followed by the discovery of those of physics, during the same century, and of those of chemistry at the end of the next one; with these fundamentals known, it became possible in the 19th century to formulate the basic concepts of geology and biology. Finally, by the end of the last

[3] Mayer, "Toward a Science of Society," pp. 165–172; "The Seven Seals of Science," *The Century Co.*, New York, 1927.

century, the ground work seemed fairly complete to undertake a fruitful search for the essential variables of mental activity. The development of those of social relations would appear to be next in order.

The discovery or formulation of primary concepts is clearly tied up with the dissipation of long-standing misconceptions and with the development of specialized analytical and observational tools and techniques. Mathematics is sometimes called the "queen of the sciences" because the instruments it formulates have proved fundamental to all other analytical and observational procedures. The mysticism which surrounded the idea of numbers for a long period of time, and which had to be dispelled before a rigorous mathematical discipline could emerge, need not concern us here.[4] Suffice it to say that, with this purification and with the foundations laid by the ancient Greeks and the Hindus, the preparations were complete for the development of modern science. Without these foundations, especially the Greek analysis of conic sections, Kepler could not have discovered the laws of planetary behavior, which in part provided the elements for the modern science of astronomy. Without them, analytical geometry and the calculus as tools for depicting and analyzing motion could not have been devised, and these and other mathematical implements were likewise fundamental to an understanding of the laws of celestial and terrestrial action, worked out by Kepler, Galileo, Newton, and others. Modern mathematical analysis has gone still further and has developed additional instruments applicable not only to the newer problems of physics and chemistry but also, through a better understanding of probability, statistical inference, and mass action, to problems in biology, psychology, and the social studies.

In astronomy and physics there were innumerable misconceptions which had to be dissipated before scientific variables and hypotheses in these fields could come. Such were the long-standing ideas regarding perfect heavenly spheres moving in circular orbits about a central earth and fallacies regarding falling

[4] *Cf.*, Tobias Dantzig, "Number: The Language of Science," *Macmillan*, New York, 1933, pp. 37–56.

bodies and the influence of goat's blood or garlic on magnetic attraction. Along with the dissipation of such preconceptions came the perfection of certain physical tools which, added to the mathematical techniques, were essential to the formulation of additional primary variables. The telescope made it possible for astronomers to turn to physical celestial problems; the spectroscope enabled them further to widen their interests.[5]

As a prelude to the discovery of the element, the atom, and the molecule as basic for chemical analysis, physics had to make clear the meaning of such factors as weight, temperature, and pressure, and to develop such tools as the balance, the thermometer, the vacuum pump, the electric furnace, and the mechanism of electrolysis. And additional false theories of ancient vintage, having to do with the practices of alchemy and of the so-called imponderables, had to be swept away.

Nature study, the forerunner of the sciences of geology and biology, remained enmeshed in false conception and superficial observation until the 17th century, when the development of the microscope as an observational implement pointed the way to more scientific procedures. Then came the additional tools of physics and chemistry mentioned above, also compound and polarizing microscopes, and other technical physico-chemical implements for examining rigorously the phenomena of geological constitution and change, the structure of living tissue, and the character of life processes. After the beginning of the 19th century the application of these instruments and techniques assisted in clearing away false notions regarding catastrophic geological change, a cold earth fashioned out of hand, special biological creations, and an earthly existence beginning but a few thousand years back. Out of such developments, after 1830, came scientific concepts pertaining to geology, and around 1860 came the discovery of the cell, protoplasm, and the germ layers, and the enunciation of the evolutionary hypothesis, as the essential variables around which biology has since reared its scientific structure.

[5] For the type of technical physical and chemical instruments used in a modern astronomical observatory, see Mayer, "Seven Seals of Science," pp. 265–267.

Mystical speculation and superficial introspective observation were all that mental philosophy (the predecessor of psychology) had to work with until physics and biology provided an understanding of the physico-physiological basis of psychological phenomena. The 19th century was well under way before physicists and physiologists furnished a scientific analysis of the construction and functioning of the eye and the ear, devised suitable instruments and techniques for measuring biological changes accompanying sensations of various kinds (such as changes in blood pressure, respiration, muscular reaction, glandular response, etc.), and traced the positions of sensory and motor nerves. Later, other sense organs, the brain, the spinal cord, and the nervous system were more fully explored, and comparative and genetic bio-psychological studies were undertaken. At the same time, crude introspective, structural, faculty, and mystical preconceptions were foresworn and so-called behaviorism took its rise. Unfortunately the attempt to oust rampant metaphysical and theological misconceptions with respect to mental phenomena and to copy the 'objective' methods of physical science, has led for a time to the almost total neglect of the most important aspects of psychological phenomena as such; but with the launching of the gestalt hypothesis and the development of the Würzburg and other modern schools of verified introspective observation, it appears that psychology is finally grappling with its real problem, namely, the rigorous analysis of such higher mental phenomena as ideation, memory, imagination, reasoning, emotion, attention, volition, interest, judgment, value.

Application to social study

Several methodological considerations bearing upon social study would seem to become clarified in the light of the foregoing résumé. One, though every science must ultimately formulate its own methods and techniques, these are apparently neither wholly unique nor unpredictable, for in the dependence of each science upon preceding sciences or disciplines[6] it borrows and develops many of the elements of its primary procedures and basic

[6] Mayer, "Toward a Science of Society," pp. 165–172.

variables. Two, the sciences nearest of kin to the social studies, and thus next preceding them in the hierarchy of the sciences, are presumably psychology and biology, so that it is fair to assume that in these two fields primarily (rather than in geology, chemistry, physics, or mathematics) are to be sought the most important elements for formulating the essential techniques and concepts of social study. Three, in the light of these most important elements (whatever they may be), mathematical, statistical, and physical-science procedures in social study appear to be relegated to the plane of subsidiary aids. Four, 'genetically' considered, it would seem reasonable to seek certain of the basic variables of social study on the one hand, in those bio-psychological factors which distinguish *homo sapiens* from other animals (such as the development of the hand, language, abstract thought, and rational foresight, though the *human* is not necessarily synonymous with the *social*), and, on the other hand, in those earliest prehistoric social groupings of which we thus far have knowledge (such as the family, the clan, hunters, fishers, navigators).[7] Five, added to the 'genetic,' the historical analysis of cultural forms should throw further light upon the nature of basic social categories. Six, such an approach as the foregoing, coupled with an unbiased examination of handed-down preconceptions, has served to clear away the worst of the underbrush of fallacious and inadequate theorizing in the physical and the biological sciences; and there is no reason why similar methods cannot serve to produce similar results in the social fields.[8]

Scientific concepts and techniques for social study should, in short, be looked for: first, in psychology (especially in social psychology); secondly, in biology (as in anthropology); thirdly, in geology (as in human geography); fourthly, in the physical sciences and mathematical disciplines (as in those branches dealing with mass action, probability, statistical inference, and functional analysis of many interrelated variables). Statistical

[7] *Cf.*, J. J. M. de Morgan, "Prehistoric Man: A General Outline of Prehistory," *Knopf*, New York, 1925; G. F. Ranard, "Life and Work in Prehistoric Times," *Kegan Paul*, London, 1929; W. H. R. Rivers, "Social Organization," *Kegan Paul*, London, 1924.

[8] *Cf.*, Pitirim A. Sorokin, "Contemporary Sociological Theories," *Harpers*, New York, 1928.

analysis and mathematical manipulation of social and economic data would seem to represent necessary but preliminary steps in an understanding of social phenomena, *until* the basic social categories have been disclosed and the bio-psychological human values, interests, drives, dispositions, and the like, *which give rise to the social phenomena*, have become clarified. *Then* the statistical and mathematical aids to accurate observation of social activity in the mass, take on added significance as *verifying procedures*. But, even then, the more important observational and verifying instruments will, it appears, continue to be psychological in character, that is, instruments which lead to a clearer scientific understanding of human nature, its drives, dispositions, judgments, interests, and values. Such procedures must, perforce, be in large part 'introspective' or 'self-observational' in the best and most exclusive meaning of these terms, verified repeatedly by other self-observers and so devised that they may be as 'objective' as possible in the sense of producing unbiased and impartial results.[9] As for historical techniques, they also, like the mathematical and the statistical, must apparently be regarded as subsidiary to the main procedures indicated above, in the organization of social study into social science. The further bearing of these preliminary observations will be obvious as we proceed.

II. NATURAL-SCIENCE AND MATHEMATICAL TECHNIQUES IN SOCIAL STUDY

A few words will here be in order regarding an excellent case book "compiled under the direction of the Committee on Scientific Method in the Social Sciences of the Social Science Research Council," and containing 52 analyses of more than 60 conspicuous contributions. This work is without question the most significant and authoritative attempt thus far made in the United States to ascertain important current methods and concepts in use in the social studies, in which are included the fields of statistics, human geography, anthropology, psychology, social psychology, history, politics, law, economics, and sociology.[10]

[9] *Cf.*, *Infra*, pp. 441–442; also, Mayer, "Scientific Method and Social Science."

[10] "Methods in Social Science," edited by Stuart A. Rice, *University of Chicago Press*,

The word 'methods' in this book is used to cover not only scientific method in the broad sense of verified observation and theoretical analysis referred to at the beginning of the present article, but also specialized techniques in the narrower sense.[11] In selecting case studies of methods thus broadly and narrowly defined, seven societies represented in the membership of the Council made recommendations through advisory committees appointed for the purpose. Competent specialists were called upon to make the analyses recommended. And other precautions were taken to "include all of the principal ways of conceiving their tasks which social scientists have adopted" in providing the present structure of the social studies, "securing representation among the projected analyses for all of the more important 'schools' and points of view within the specialized disciplines."[12]

The arrangement of the case studies in the volume in question is more or less arbitrary, and various suggestions for regrouping them are contained in the appendices. Their rearrangement, in the light of our preceding discussion, and an outline of their content, will serve to give further point to the present account.

Relationship of social study to recognized sciences

With respect to the relation of the social studies to the recognized sciences, two analyses are included in the case book: one

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Chicago, 1931. A number of reviews of this work have appeared, among which the following may be cited: a German apprasal by Karl Mannheim and a French comment by Maurice Halbwachs, *The American Journal of Sociology*, Sept., 1932, pp. 273–282, Nov., 1932, pp. 453–458; also, Benjamin Ginzburg, *New Republic*, March 11, 1931, pp. 103–104; S. W. Fernberger, *Ann. of Am. Acad. Polit. and Social Sc.*, May, 1931, p. 243; C. M. Perry, *Int. J. of Ethics*, July, 1931, pp. 524–526; W. J. Shepard, *Am. Pol. Sc. Rev.*, Nov., 1931, pp. 1071–1073.

[11] "Methods in Social Science," pp. 7–10. The "conceptual version" of method ("the concepts and assumptions underlying scientific inquiry," "ways of conceiving problems and data," "the set of ideas" which an investigator uses, his "guiding point of view," etc.) is here regarded as more fundamental and precedent to method in the more limited sense of specialized techniques and procedures. With this statement the present writer is in agreement except that he would point out that the conceptual phase of general scientific method must be supplemented by the observational phase. If the editor of the case book means to suggest that, at the present stage of social study, conceptual problems are more important than observational problems (the techniques of which are fairly well advanced), the present writer would be inclined also to agree.

[12] *Ibid.*, pp. 736–737, 743.

bearing on the profound methodology of Auguste Comte; the other sketching certain connections within modern psychology and between psychology and other sciences.[13] Comte emphasized the hierarchy of the sciences, indicated that the scientific organization of the social studies must be based upon a fuller knowledge of biology, and attempted in a preliminary way to distinguish between psychological, sociological, and ethical interests.[14]

Psychology as a scientific discipline had not been created in 1857 when Comte died, so that his impressions regarding mental phenomena should be supplemented with present-day knowledge. This the second analysis mentioned attempts to do, albeit, it would seem, with unnecessary inconclusiveness.[15] In it the experimental, structural psychology of Titchener as a pupil of Wundt is compared with the psychology of gestalt, which emphasizes the form or configuration of mental phenomena; and the methodology of Titchener in ranking psychology with biology and physics as one of "the three major sciences," is indicated. But the significance of this methodology is not pursued by the case-book analyst, nor are the important modern developments of the Wundt-Titchener techniques even mentioned.[16]

Comte (to return to the case-book analysis of his work), in his so-called "law of the three states," gleaned from an exhaustive study of the history of the sciences, gave pointed emphasis to the conceptual difficulties confronting any field of knowledge which is endeavoring to organize itself into a science. "From the nature of the human intellect, each branch of knowledge in its development is necessarily obliged to pass through three different theoretical states: the theological or fictitious state; the metaphysical or abstract state; and lastly, the scientific or positive state."[17] Thus, if social studies are still encumbered with theological and metaphysical preconceptions, it is probably merely symptomatic of a period in their development toward scientific maturity.

[13] *Ibid.*, analyses 1 and 5.

[14] *Cf.*, Mayer, "Toward a Science of Society," pp. 165–172.

[15] *Cf.*, Fernberger's review, *Annals*, May, 1931, p. 243.

[16] *Cf.*, "Methods in Social Science," analyses 51 and 52, for these developments.

[17] Comte, "Early Essays on Social Philosophy," translated by Hutton, *Routledge*, London, 1911, pp. 131–132.

Comte also recognized that a scientific understanding of the basic variables of human nature is a necessary preliminary to the organization of sociology into a science. Man's "spontaneous tendencies" can alone "furnish sufficiently solid basis for its authority."[18] One must "begin by analyzing the individual impulsions which are the characteristic elements of the progressive power of the human species," and must relate "these impulsions to that fundamental instinct (the eminently complex result of the necessary concourse of all our natural tendencies) which directly urges man to ameliorate his condition."[19] Comte further insisted that until such fundamentals have been clearly formulated, it is futile to assume that fragmentary studies of social relations will be anything but superficial and misleading. In the meantime one will obtain "only a sterile and encumbering mass of irrational special discussions, badly instituted and worse pursued."[20]

And, one might add, no amount of precise mathematical or statistical manipulation of any such sterile mass can throw light upon the characteristic 'impulsions,' 'fundamental instincts,' or 'natural tendencies' of the human species. Much has been written about 'instincts' and 'tendencies' since Comte's time, but students of social relations today seem to require, about as much as they did then, the exhortation he gave regarding the essential need of ascertaining the basic variables of human nature.

Bio-psychological fundamentals and techniques

What psychology, biology, and 'behaviorism' have in recent years been able to contribute regarding these human-nature fundamentals seems regretably meagre if measured by the concepts agreed upon among specialists in these fields. The pioneer work of McDougall on primal human instincts is still as vigorously criticized as it is defended, with little apparent disposition to accept his methodology even tentatively and with open-minded-

[18] Comte, "Cours de philosophie positive," edited by Littré, 2nd edition, Baillière, Paris, 1864, IV, p. 252.

[19] *Ibid.*, p. 262.

[20] *Ibid.*, pp. 255–256.

ness to test out his preliminary hypotheses more thoroughly.[21] Possibly now that the tide in favor of crass behaviorism has turned and one may venture to espouse rigorous introspective analysis without being consigned to the outer darkness of faculty ghosts and supernatural demons, more comprehensive and constructive analyses of elementals will be attempted. A great mass of observational data has already accumulated. What is here apparently needed primarily is better sifting and synthesis in the light of more adequate hypotheses in which the introspective as well as the behavioristic is assigned a proper rôle. Briefly stated, McDougall's contention, based upon many years of fact-gathering and experience as a recognized psychologist, is that man possesses a limited number of native instinctive dispositions (psychophysical in nature, with cognitive, affective, and conative aspects); which as such have undergone virtually no change in the history of the race; out of which through acquired experience arise the compound sentiments or emotions and the deliberate volitions that constitute individual character; and upon which all social conduct must ultimately rest.[22] This is not the place to enter into the controversies McDougall's theory has engendered. To the present writer his theory appears as an excellent working hypothesis, which might be modified in a number of directions without losing its essential significance, and which, with the casting off of behavioristic and mechanistic bewitchments, should before long be subjected to adequate testing procedures.[23]

As indicated in certain other of the case-book studies, however, great advance has been made recently in developing important psychological and behavioristic tools and techniques. There are the tests and rating scales devised by Thorndike and others, for the purpose of making comparisons or arranging an order of merit with respect to qualitative differences in individual performance.[24] There are also:—rigorous methods outlined by

[21] "Methods in Social Science," analysis 10.

[22] *Cf.*, *infra*, p. 444.

[23] *Cf.*, "Methods," analysis 9, with respect to difficulties inherent in 'objective' typological attempts to study character.

[24] "Methods," analysis 13. The distinction between 'subjective' and 'objective' procedures is discussed by the present writer in "Scientific Method and Social Science."

Woodworth of studying young delinquents, portrayed in the work of two investigators, both using psychological and statistical implements, one employing 'subjective' procedures, the other insisting upon 'behavioristic' methods; certain attempts, though crude, to understand delinquent boys for purposes of reformation; strict bio-psychological studies, such as the effect of rest pauses on repetitive work; and the methods of Thurstone of estimating group influences on mental activity.[25] There are, finally, what to the present writer are the more significant introspective or self-observational techniques for the study of the higher mental processes developed in the Würzburg laboratory and by the followers of Ebbinghaus and others.[26] As already indicated, it would seem that a further development of such procedures is of highest importance for general and social psychology and for the specialized social studies. The application of rigorous self-observational techniques to economic, political, and legal problems surrounding human interests and values, should do much to advance preliminary knowledge of these phenomena pending the time when such methodology has provided a more thorough-going understanding of human nature itself,—of the drives, emotions, interests, judgments, and values which constitute its primary elements.

'Genetic' approach to social study

Supplementing the strictly psychological and behavioristic attack upon the problems of human nature is the 'genetic' attack upon the problems of existing social relations and institutions, also exemplified in the case book. Sumner emphasized this significant approach by endeavoring to reduce complex social institutions to more elementary folkways and mores.[27] After years of exploratory research, gathering material in innumerable

[25] "Methods," analyses 39, 36, 48, 49. *Cf.*, also, L. L. Thurstone and E. J. Chave, "The Measurement of Attitude," *University of Chicago Press*, Chicago, 1929; Stuart A. Rice (ed.), "Statistics in Social Studies," *University of Pennsylvania Press*, Philadelphia, 1930, pp. 171–196.

[26] "Methods," analyses 51 and 52.

[27] *Ibid.*, analysis 8.

fields and languages for the analysis of existing cultures to derive their typical elementary forms, Sumner attempted to uncover those phases of human nature and behavior which lead to social custom and habit. He viewed folkways as cultural traits or action patterns, the products of use, wont, manners, and customs, which show the diversified ways of ordinary human action and the forms of belief and sentiment characterizing social life, the immediate motivating force behind them being human interest. Mores he regarded as special types of folkways, having to do with what is right and proper: "All notions of propriety, decency, chastity, politeness, order, duty, right, rights, discipline, respect, reverence, cooperation, and fellowship, especially all things in regard to which good and ill depend entirely on the point at which the line is drawn, are in the mores."[28]

At the same time, folkways and mores were not thought of by Sumner as "creations of human purpose and wit. . . . They are like products of natural forces which men unconsciously set in motion, or they are like the instinctive ways of animals, which are developed out of experience, which reach a final form of maximum adaptation to an interest, which are handed down by tradition, and admit of no exception or variation, yet change to meet new conditions, still within the same limited methods, and without rational reflection or purpose."[29] The present writer, however, questions whether any such clear-cut line of demarcation can be drawn between unconscious and deliberate human activity.

Some of the social categories which Sumner found important as a result of a life-long study of his materials, are the following: the family group; other groups or classes; group character or ethos; racial and nationalistic sentiments or ethnocentrism; religious sentiments and sectarianism or asceticism; taboo, convention, and etiquette; fads and fashions; ceremonial and literature; institutions.[30] The relative importance and potency of these preliminary or exploratory concepts, or of like social categories, must rest upon the characteristic pattern of funda-

[28] William Graham Sumner, "Folkways: etc.," *Ginn*, New York, 1906, p. 231.
[29] *Ibid.*, p. 4.
[30] "Methods," pp. 160–161.

mental innate capacities or dispositions in human nature out of which the folkways and mores take their rise.[31]

Somewhat similar in point of view to Sumner's *Folkways*, but using other methods and concepts, is the more recent study of the Polish peasant by Thomas and Znaniecki.[31a] Where Sumner talks of customs and habits, these writers speak of values and attitudes. Sumner was interested in a *description* of the folkways and mores; Thomas and Znaniecki, in *changes* when emigrants move from one set of social conditions (in Poland) to another (in the United States). In *The Polish Peasant* the life-history method is used, based upon a study of intimate family letters and the lives of Polish immigrants in the United States, and a conception of personality is formulated around which to group the attitudes and values disclosed. Three types of personality emerge from these studies: the philistine, or conventional; the bohemian, or radical; and the creative, or purposeful. As fundamental to these types, there appear four positive 'wishes' or 'desires': for new experience (curiosity); for security (caution and avoidance of danger); for recognition (self-seeking and ambition); and for affectionate response (sex craving, love, friendship). These so-called wishes or desires may be compared with McDougall's innate dispositions: curiosity and wonder; pugnacity and anger, flight and fear; self assertion and elation, acquisition and construction; parental instinct and the tender emotion, self-abasement and subjection, reproductive instinct, and gregariousness.[32]

Other more specialized 'genetic' studies of existing social relations, customs, habits, attitudes, or beliefs have also been undertaken in recent years. Among these the case book mentions significant investigations into the essential dispositions, drives, and interests underlying the development of family and kinship, horde and tribe, race, myths, and economic institutions.

Malinowski's anthropological study of sex and repression in existing savage Melanesian societies may be regarded as of highest

[31] *Ibid.*, p. 158.
[31a] *Ibid.*, pp. 162–175.
[32] *Ibid.*, pp. 191–192.

importance in pointing the way to further fundamental psychogenetic investigations.[33] Although his work, also, is largely exploratory, Malinowski raises significant questions regarding the theories of Freud, 'mother-right' and 'father-right' complexes, origins of horde, clan, and tribe, and the beginnings of culture in general. The creation of implements and language and the formation of the primitive family are apparently among those fundamentals upon which the development of human nature and of social or cultural relations has depended.

The studies of Boas into the origins of race and of myths, if not possibly as basic 'genetically' as those of Malinowski, are probably even more important from other points of view, especially in the rigor of the techniques employed.[34] His carefully-controlled anthropometric investigations of the physical characteristics of American immigrants, have made it strikingly clear that racial types change materially under new physical environments and have opened the way to strictly scientific studies of heredity and race mixture. It has never been more apparent "that there are today no pure or unmixed races" and that "there is no correlation between race, language, nationality, or religion."[35] Race, therefore, is probably not an elementary human or social category.

The folk-tale study of Boas and his students is "the most voluminous and careful analysis of any primitive literature known to the analyst."[36] The methods employed are again empirical and comparative. They consist, for a given area, of collecting all existing comparable folk tales and of making a careful inductive analysis of them. The results are likewise significant in that they tend to dispel the preconception of a definite sequence or evolution of cultural forms. Folk myths or tales appear as combinations of disconnected culture elements, representing the play of human imagination upon every-day wishes, activities, sentiments, and emotions; brought together through unequal

[33] *Ibid.*, analysis 34.
[34] *Ibid.*, analyses 42 and 31.
[35] *Ibid.*, p. 582.
[36] *Ibid.*, p. 449.

diffusion of various components from different centers and through their accidental commingling; and reintegrated secondarily with local elements. Here is an excellent field and methodology for further significant investigations into the genesis and development of certain folkways, on the one hand, and of the imaginative mental life of primitive peoples, on the other. . . .

Hobson's effort to bring about a valuation of economic phenomena in terms of human needs, interests, and welfare is not only a 'genetic' investigation of human motives in economics but also a penetrating criticism of abstract classical value theory and an ethical appraisal of existing economic practices.[37] While certain of his ideas, such as those bearing upon the social-organism theory, upon an out-moded psychology, or upon socialist doctrine, are open to legitimate attack, the case-book analysis falls far short of doing justice to his central position, which will be more fully explored later on.[38]

Further examples of 'genetic' studies of existing social relations have been or will be mentioned in other connections.[39] Together with the more strictly bio-psychological analyses already taken up, they strive to answer questions of first importance for a more scientific understanding of human nature and social phenomena. Until, through such studies, more or less satisfactory answers are provided and until the innate drives and acquired tendencies of the human organism are more clearly discerned, any direct approach to complex social facts and processes is, as Comte indicated, bound to be superficial and exploratory, even as biological facts were inadequately understood before the evolutionary hypothesis was launched and verified.[40]

Bio-geological approaches

While biology should ultimately assist students of society to a better understanding of the nature of man, it and its sister science

[37] *Ibid.*, analysis 35.

[38] *Infra*, pp. 479–480.

[39] *Cf.*, analyses 32, 39, 40.

[40] *Cf.*, also, suggestion in Woodworth's letter ("Methods," p. 744) regarding the importance of studies in child behavior as bearing upon the genesis of social relations. Such studies are not dealt with in the case book.

geology also offer the use of their techniques for reconstructing prehistoric cultures and for indicating the conditioning rôle of natural environment. In the one respect, the case book provides excellent examples of the use of stratigraphic, archeological, and related criteria in helping to establish cultural sequences, to determine prehistoric culture areas, and to trace their interrelations.[41] The more extensive pursual of such pioneer investigations should lead to important knowledge of the material aspects of primitive folkways and mores and of the more complex cultures which marked the beginnings of civilization.

In the other respect, the case book reveals the conditioning influence of natural environment at all stages of cultural process from prehistoric to modern times.[41a] Satisfactory techniques for appraising this influence with regard to any given culture or culture-area are still to be worked out, though, here too, significant exploratory efforts are in progress. The best example afforded by the case book of environmental studies of early cultures has to do with Wissler's anthropological researches among primitive peoples, leading to his development of the concepts of culture-area and age-area.[42] These concepts, borrowed from systematic biology and natural history, "deal with the space distribution of culture phenomena";[43] though not new in anthropologic research, they were without explicit methodological formulation before Wissler's writings. In investigating American Indian cultures, Wissler divided the hemisphere into a number of culture areas and listed the principal traits characterizing each, such as physical types, foods, languages, flora and fauna, climate. In the culture-area, a center or nucleus of diffusion is emphasized and the nature of changes at the fringes is pointed out. The age-area concept attempts to establish relative time-sequences from different survival qualities of culture-traits at the center and periphery of culture areas. The chief criticism thus far against

[41] "Methods," analyses 18 and 19.

[41a] *Cf.*, L. P. V. Febvre, "A Geographical Introductory to History," *Kegan Paul*, London, 1925.

[42] "Methods" analysis 17.

[43] *Ibid.*, p. 248.

these concepts seems to be that they tend to be used apart from other procedures and, thus isolated, lead to questionable results. Combined with psychological, 'genetic,' and historical techniques, they give promise of bringing about a more satisfactory understanding of the relation of primitive cultures to environment.

Less rigorous inventory or survey methods for preliminary investigations of the topography, natural resources, economic and industrial conditions in small geographic areas are being applied to modern communities.[44] Of much more significance here are the limited but carefully projected studies in agricultural economics instituted in recent years.[45] An outstanding example of such investigations, involving also the use of statistical procedures, is the study by Ezekiel of the concept of diminishing returns from natural resources, a study of essential import in depicting the bearing of conditioning natural-environment forces upon economic relations.[46]

Mathematico-statistical methodology

The case-book methods and procedures thus far outlined (the psychological, biological, and geological), though fundamentally important for the scientific formulation of the basic concepts of human nature and social relations, have been viewed primarily as they apply to the intimate study of individuals or of severely limited social groups or of geographic areas.[47] Where relations between great masses within large areas and involving an indefinite number of variables are concerned, as is the case with the complex institutional situations of modern societies, it is necessary to apply, both at the preliminary and at every intermediary stage of observational verification, physical and mathematical techniques having to do with such subjects as mass action, probability and chance, functions and functionals. These preliminary and intermediary applications temporarily reduce

[44] *Ibid.*, analyses 14, 15, 16.

[45] *Ibid.*, Appendix F.

[46] *Ibid.*, analysis 45.

[47] The exceptions, e.g., in analyses 39 and 45, employ the additional statistical techniques about to be examined.

individuals and social variables to things, numbers, and functional relationships, but this is a necessary procedure if complex relations and variables are to be kept logically before the mind. It must not be forgotten, however, that these applications are always preliminary or intermediary and that the ultimate purpose for scientific social study must be to make the primary psychological, biological, and geographical analyses mentioned in the foregoing pages. One is otherwise taking the means for the end, the shadow for the reality, the shell for the content, and may even go so far as unwittingly to subscribe to a "holy and immaculate quantitativism."[48]

These precautionary remarks are not meant as a derogation of physical-science and mathematical methodology in social study. They are meant merely to indicate its place. In its place it is highly important and must always continue to be so, if anything increasingly so after the basic concepts of social study have been formulated. At present, its importance rests upon the working out of technical procedures particularly applicable to social study, and in this respect remarkable progress has been made in recent years, in statistics on the one hand and in higher mathematical analysis on the other.

Mathematical and statistical methods in social study are sometimes combined. The latter depends upon and acquires more rigorous meaning through the former, although one may make use of statistical formulae and tables without understanding the mathematical analysis upon which they depend. Such is the usual situation. At times, however, as in the pioneer work of Moore, the application of higher mathematical analysis to social problems is direct and primary, though statistical procedures may likewise be involved.[49] Moore's important work has to do with developing mathematico-statistical formulae covering the axiomatic, common-sense demand relationship, namely, that "the price of goods varies directly as the quantity demanded and

[48] *Cf.*, Sorokin, "Recent Social Trends: A Criticism," *The Journal of Political Economy*, April, 1933, pp. 194–210.

[49] "Methods," analysis 46. *Cf.*, also, G. C. Evans, "Mathematical Introduction to Economics," *McGraw-Hill*, New York, 1930.

inversely as the quantity supplied."[50] This broad statement, accepted by economists a century ago as all sufficient, was subjected to successive refinements and clarifications (by Cournot, Walras, and others), as the interdependence of all prices and their relationship to time came to be appreciated, until a theoretically-complete formulation of the demand function in all its complexity was obtained. In place of the ambiguous $P \propto \frac{D}{S}$ we now have the precise $D = f(p_1, p_2, \ldots\ldots, p_n, t)$. With this general formulation as a basis, Moore set himself the problem of so modifying the function, without materially disturbing its rigor, that by the application of certain statistical devices (multiple correlation, relative changes, and trend ratios) he might be able to secure concrete demand equations from actual market prices. This problem he has solved in his formulation of statistical demand curves for corn, potatoes, cotton, and other commodities. Moore's mathematico-statistical contributions are important and noteworthy, but the very description of them indicates the severe limitations of the methods employed. Even with a few simple variables, each of which is precisely defined (quantity, price, time), the procedures are difficult to apply. To the complex and exceedingly intangible phenomena of the mind, the folkways, and other social processes described in preceding pages, they are as yet entirely inapplicable. In their place, however, and in their bearing upon the development of statistical techniques which have a fairly wide though still somewhat limited application, they are of highest significance for social study.

Statistics deals with large aggregations of data, often called 'populations,' which are variable and which it is sought to reduce to manageable form. The critical selection and assembling of raw statistical material is the first problem; then follows the reduction of the material to a few relevant or significant values, which consistently, efficiently, or sufficiently reflect not only the selected sample but the whole population of which it is a part. Techniques of sampling, probability, and error are important first and last; and, in different stages of statistical handling, such

[50] "Methods," p. 645.

additional devices are utilized as graphic portrayal, arrangement into frequency or time series, and the calculation of means, deviations, coefficients of correlation, index numbers, and the like. At bottom, then, statistics deals with variable traits or characteristics (such as the height of soldiers) which are quantitatively measurable and thus comparable. But no matter how perfectly the handling is carried through, it can rise no higher than its source. If the traits making up the 'population' are superficially chosen, the results of the statistical analysis can have no deeper significance for social science than a superficial one. If the traits are selected with exploratory intent, the statistical results remain exploratory. Only insofar as the traits chosen represent basic variables in human nature and culture (which, of course, means that pertinent psychological, biological, and geological techniques have previously been applied), can the results of statistical manipulation be regarded as having real scientific import. And how far such basic categories (when formulated) will be found to be quantitatively measurable is another question the answer to which cannot be determined in advance. Qualitative considerations still appear of greater significance in social relations than quantitative ones.

The case book contains a number of excellent examples of statistical method applied to social factors selected in a superficial, preliminary, exploratory, or more rigorous manner, with or without the assistance of more fundamental social-science procedures. Brief outlines of ten such analyses follow.[51]

The first covers investigations by Ogburn and Burgess which demonstrate the uses, difficulties, limitations, and superficial character of purely-behavioristic concepts no matter how painstakingly analyzed statistically.[52] The raw concepts used are the vote or ballot "as an index" of attitudes or opinions; certain overlapping 'social classes'; 'conservatism' vs. 'radicalism'; and parole violations.[53] The analyst, Stuart A. Rice, indicates that,

[51] *Cf.*, also, analyses 39, 45, and 46, already reviewed in other connections.

[52] "Methods," analysis 43.

[53] *Cf.*, *Ibid.*, pp. 75, 84–86, for Elliott's criticism of use of such superficial notions as "vote" and "radical-conservative" antithesis.

though such crude factors are thought of as 'objective,' the choice of the topics studied, the type of data selected, the sampling processes employed, and the inferences drawn, are clearly conditioned by highly 'subjective,' biased, fortuitous, and predetermined considerations.

The second covers an analysis of studies into the causes of non-voting, using experimental and control groups and the necessary statistical techniques but including such vague and inconclusive 'objective' tests as 'economic status' (based only on rentals), 'literacy' (based wholly on the reading of English), 'conditions of schooling,' and 'knowledge of government.'[54] The view of the analyst of these studies, that such investigations are 'scientific,' may be compared with the view of Rice with respect to the Ogburn-Burgess studies just referred to.

The third furnishes an additional illustration of misunderstandings by another analyst of the meaning of scientific method and of such processes as 'induction' and 'deduction.'[56] This analysis exemplifies, also, the use of important statistically-summarized factual studies in giving more definite meaning to superficial common-sense assumptions (as Marshall's 'representative firm' which, as one might expect, turns out to be the statistical mode), and the tendency to assume that fundamental social research is thus being pursued when the net result is the statistical elaboration of the obvious.

The fourth covers a Missouri crime survey in which carefully-devised statistical and descriptive procedures resulted in the gathering of significant data bearing on the administration of criminal justice in that state.[57] The variables employed proved, however, to be wholly inadequate, either in revealing the causes and remedies of crime or in determining the efficiency of the administrative system, both of which considerations were the avowed objectives behind the survey. Sutherland's excellent analysis of these shortcomings is well worth careful reading.

The fifth covers Shaw's studies of juvenile delinquency, which

[54] *Ibid.*, analysis 50.

[56] "Methods," analysis 44. *Cf.*, the footnotes of this analysis referring to the reactions of Deibler and Opie to the views of the analyst.

[57] *Ibid.*, analysis 38.

illustrate the employment of adequate statistical, case-method, life-history, and interview techniques in exploratory and confirmatory investigations, but which define the concept 'delinquent' loosely and indefinitely and apparently fail to apply any precise psychological, 'genetic,' or environmental criteria except as these are to some extent implicit in the author's general experience and outlook.[58]

The sixth covers a highly technical and efficient statistical analysis of a long series of British and American business-cycle and related observations made in an attempt to show significant correlations between periods of prosperity and depression and such other social variables as marriage, prostitution, birth and death rates, pauperism, alcoholism, crime, and emigration, all of these concepts being highly complex, poorly defined, and vaguely conceived. Both the limitations of the procedures thus applied and the superficial nature and "doubtful validity" of the concepts are indicated in the analysis.[59]

The seventh covers the carefully-devised methods employed in taking the federal census of population in the United States, with the necessary use of very general or superficial categories (color, race, nativity, sex, age, occupation) so far as basic social variables are concerned.[60]

The eighth presents a technical improvement in a statistical device based upon inductive studies but dealing with the crudely broad concepts of family expenditures in the United States for food and for general maintenance.[61]

The ninth covers Thrasher's study of gangs, a highly significant exploratory investigation using psychological, behavioristic, 'genetic,' environmental, statistical, survey, case-study, and interview techniques in an effort to determine basic personal and cultural categories.[62] The analyst, Kimball Young, points out that, even in such rigorous investigations, statistical analysis and mere quantitative considerations are "at present inadequate

[58] *Ibid.*, analysis 40.
[59] *Ibid.*, analysis 41.
[60] *Ibid.*, analysis 11.
[61] *Ibid.*, analysis 12.
[62] *Ibid.*, analysis 37.

to reveal the factors" which are basic, both because of "the overwhelming number and complexity of the variables involved" and because a mechanistic (behavoristic) approach does not expose the inner mental processes.[63]

The tenth covers Mitchell's extensive statistical investigations of the business cycle—probably the outstanding examples anywhere of a scientific attack upon a complex social problem, which, if not a basic variable of human nature or of primary folkways, is undoubtedly one of the most important factors in modern capitalistic society.[64] The attack moves from a general exploratory to a more rigorous approach in ascertaining the meaning of business cycles, preconceived theories being impartially examined in the light of far-reaching statistical analyses and with the assistance of a competent technical staff, judgment being withheld regarding underlying causal connections, and traditional abstract economic theories being carefully avoided. The tentative conclusions are couched in terms of "analytic description" and are thought of as merely marking stages on the road to further investigation. Mitchell's immediate problem is a clearer definition of the complex business-cycle concept. Until the basic categories of social science are formulated, causal relationships between them and the social problem in hand cannot, of course, be arrived at. In the interim, the only proper approach to intricate institutional problems would seem to be the one pursued by Mitchell. His own view of the necessity for such an approach is given in the appendix to the case-book analysis, in a letter which in the opinion of the present writer should become historic. All students interested in the application of scientific method to social phenomena will profit by a careful reading of this excellent discussion.[65]

III. HISTORICAL AND RELATED CRITERIA IN SOCIAL STUDY

The Methods of the Historian

The social-study techniques and procedures thus far described have been developed by the recognized scientific disciplines from

[63] *Ibid.*, pp. 520, 523.
[64] *Ibid.*, analysis 47.
[65] *Ibid.*, pp. 675–680.

mathematics through psychology. They may be applied to social relations either now in being or once existent in the past, although our review up to the present has been concerned chiefly with the former rather than with the latter. In dealing with past events the additional technical problem arises of resurrecting as accurately as may be the factors that are gone. This can obviously never be completely realized, but insofar as it is possible of accomplishment further procedures, the historical, are called into use.

A number of the case-book analyses bear directly on such techniques, those by Pirenne and Schevill being outstanding. Pirenne outlines what historians are trying to do.[66] He indicates the severe limitations of historical method; the essentially fragmentary character of historical records, whether unwritten or written; the documentary procedures developed in epigraphy, paleography, diplomatics, numismatics, sigillography, heraldry, and the like; the constant need for technical judgment as to authenticity and credibility; the conjectural character of the historian's point of view as he constructs history from the inexact and patchy vestiges with which he works; the changing preconceptions or hypotheses (supernatural, great-man, political, social, economic, and other explanatory theories), as knowledge of past events broadens; the necessity for making comparative and 'objective' studies so far as possible if the narrative is "to appear in its true perspective" as universal history.[67] All this is excellent from the point of view of the historian as such. But for social science it is not enough. Pirenne himself gives the clue to the inadequacy, although he does not follow it up. All historical construction or narration, he says, rests upon the postulate "of the eternal identity of human nature." The historian assumes "he can treat the actions of the dead as he does those of the living who surround him."[68] If that be true, is not at least part of the fundamental orientation for history, as for social science, to be sought in those eternal invariants of human nature? Where else

[66] *Ibid.*, analysis 30.
[67] *Ibid.*, p. 445.
[68] *Ibid.*, p. 442.

is the proper 'frame of reference' for past and present social events to be found?

Schevill strikes this deeper note in his superb portrayal of the methods and point of view of Voltaire, who in the 18th century emphasized the importance of man and his environment in the making of history.[68a] Beyond these essentials, Voltaire thought of historical narrative in terms of "literature, facts, and philosophy."[69] Employing a clear, witty, and sparkling style, and inspired by the naturalism of the physical-science developments of his day, he passionately fought the prevailing supernaturalism, minimized the influence of religion and politics in history, and stressed intellectual, social and economic factors. Following him but disagreeing with him, the Romantics placed their full faith in "historical continuity." In the 19th century the need was felt for "a richer and better-accredited corpus of facts," and the ancillary specialties of diplomatics, paleography, etc., took their rise.[70] Today, with richer and better facts before us, there is a return to the spirit of Voltaire,—a demand for "a new synthesis involving a new picture of the human whole."[71]

Many historical questions have to do with the problem of social change and causation. Here Voltaire had not made up his mind. "What can we say in conclusion," says Schevill, "but reaffirm that Voltaire had not thought this matter of causation through? Having broken with the Christian theory of a supernatural agent he launched our historical craft upon a disturbed sea of speculation, on which, in spite of the occasional cry of 'land' on the part of a passenger regularly found on investigation to be suffering from obstinate and confirmed astigmatism, it continues to toss irresolutely to this day."[72]

After Pirenne and Schevill, the rest of the case-book analyses on historical method seem to strike a somewhat minor though not inharmonious key. Six additional outstanding historical writers

[68a] *Ibid.*, analysis 29.
[69] *Ibid.*, p. 425.
[70] *Ibid.*, p. 430.
[71] *Ibid.*, p. 431.
[72] *Ibid.*, p. 434.

are dealt with. Michelet is shown as doing his work before the significance of economic and social factors was discerned.[73] He lived in a period of transition, but he nevertheless perceived the influence of geography upon history, stressed philosophical implications, and believed in "the self-creative power of humanity."[74] Renan, the historian of Christianity and Judaism, is exhibited as struggling between the two conflicting theories of great men and mass action as determining historical events.[75] He carried on careful research with manuscripts, coins, and inscriptions, inspected historical places, and in general used to good effect then-existing criteria of authenticity and analogy. He tended to emphasize the influence of men, rather than of races or classes, but felt that each race has a destiny and that humanity is motivated by a spiritual principle. Troeltsch, who attempted to treat historical movements (such as Christian civilization) sociologically and psychologically, emphasized the bearing of the qualitatively unique in social relations against the quantitative methods of natural science.[76] Von Below and Pirenne, in their important contributions on the origins of the medieval town, demonstrated the value of modern criteria as against earlier methods. They examined impartially four narrow and particularistic conceptions of town origins, which depended upon incomplete, relatively late sources, and, utilizing much broader and earlier material, established the mercantile character of such origins. Their more comprehensive theories stress the significance of economic and social factors in history and minimize the legal and the political.[77] Two American historians are also included in the analyses. Turner, who was profoundly influenced by the natural sciences, centered his studies of the American frontier and the fundamental attitudes of the national mind, upon the idea of an 'evolution' of cultural forms. In place of ultimate causes, he emphasized economic factors, human geography, statistical

[73] *Ibid.*, analysis 26.
[74] *Ibid.*, p. 396.
[75] *Ibid.*, analysis 27.
[76] *Ibid.*, analysis 28.
[77] *Ibid.*, analysis 24.

method, tentative conclusions, and forces that interplay in society.[78] Fay, in his *Origins of the World War*, demonstrated what a trained historian can accomplish in maintaining an impartial perspective in the midst of a tremendous mass of material, much of it highly prejudicial and dealing with controversial issues. He used technical historical procedures with excellent effect, in avoiding the pitfalls of biased secondary sources and in sizing up, with extreme fairness, significant events from 1870 to 1914.[80] In part his sources were thus contemporary, and his handling of them indicates that no student of society, whether dealing with current or past events, can afford to neglect historical tools of research. He must be well equipped with these as with the other procedures outlined.

Combined use of scientific and historical techniques

The combined use of scientific and historical procedures in significant social studies has been mentioned incidentally in connection with several of the case-book analyses already reviewed, as in Mitchell's work on business cycles. Two particularly pertinent examples will now be taken up, illustrating the difficulty as well as the necessity of maintaining a broad methodological perspective in comprehensive sociological research.

The first of these pertains to the work of Pareto, which the case-book analyst, Max Handman, outlines and evaluates to excellent effect so far as he goes.[81] The present writer would go somewhat further. To him, Pareto appears as the lineal methodological descendant of Francis Bacon, attempting by similar means to develop a new and one-sided school of thought, but at a time when the general occasion which gave point to the earlier attempt has long since passed and in a way which fails to take account of "probably more effective schools of modern psychopathology" and of other equally important procedures.[82] Like

[78] *Ibid.*, analysis 23; *Cf.*, also, B. F. Wright, Jr., "American Democracy and the Frontier," *The Yale Review*, Winter, 1930, pp. 349–365.

[80] "Methods," analysis 25.

[81] *Ibid.*, analysis 7.

[82] *Ibid.*, p. 153.

Bacon, Pareto shows speculative power and imaginative insight, while at the same time discounting these faculties; he is passionate and ironic in his utterances, while demanding pure objectivity in others; he lays exclusive emphasis upon logic and induction and ridicules preconceived notions, while setting forth, without inductive extractions, conceptions as metaphysical and *a priori* in character as any he criticizes.[83] Whereas in one of his so-called 'derivatives,' Pareto scoffs at the use of analogies (such as statics vs. dynamics) of "purely verbal manipulations," or of words with adopted connotations, he nevertheless borrows from physical mechanics most of his important concepts—action vs. reaction, real and virtual motions, an equilibrium of forces—and, with respect to others, employs high-sounding terminology to portray common ideas that are vague and undefined.[84] His formalistic logical symbols to represent familiar notions are in fact so subjective and hazy that almost any broad meaning can be assigned to them at will. For example, he endows man with certain instinctive forces, which he calls 'residues,' "and later on" says Sorokin, "deduces from them whatever he likes."[85]

Pareto resolves most of the complex individual and social problems that confront mankind into processes of mechanical action and reaction. He thinks in terms of bifurcate, antithetical, energy categories. To him the 'residues' represent a group of human drives or dispositions making for non-logical action, against which the 'derivatives' represent logical efforts to mask or to hide the non-logical drives. The 'residues' he regards as six in number, conceived in terms of two antithetical pairs and two others. Stripped of formalistic titles, these are: (1) a disposition to combine or put together all kinds of things and ideas (compare Veblen's instinct of workmanship and Hume's association of ideas); (2) its opposite, "to refrain from tearing asunder" what has been put together (compare the inertia and persistence of habit and custom); (3) a disposition in people "to get together

[83] *Cf.*, Mayer, "Seven Seals of Science," pp. 93–95.

[84] *Cf.*, "Methods," pp. 144–148; Sorokin, "Contemporary Sociological Theories," pp. 37–62.

[85] Sorokin, *Ibid.*, p. 61.

with others" and form groups (compare familiar ideas of gregariousness or consciousness of kind); (4) again, in antithesis, a tendency to preserve the collectivity or keep the social relations intact; (5) an inner necessity for external action or expression (apparently in general); (6) a disposition to act sexually. The 'derivatives' or tendencies to rationalize irrational behavior he thinks of as four in number: (1) simple or repeated affirmation; (2) authoritative utterance; (3) consensus of general opinion; (4) verbal manipulation, play on words or sentiments, and use of vague and indefinite terms. In these 'derivatives,' Pareto follows Bacon's earlier suggestions almost completely[86] and adds nothing from modern psycho-analytical researches, such as those of Freud, which have thrown so much light upon the nature of rationalization processes. As for his notion of human-nature essentials (his 'residues'), these may be further simplified in terms of tendencies to put things together and to get together, to keep things together and to stay together, to act and to act sexually, which ideas may be compared with the more rigorously derived 'instincts' of McDougall or the four 'wishes' or 'desires' of Thomas and Znaniecki.[87]

Pareto 'feels' that there is in the nature of things some kind of individual and social "necessity for a state of equilibrium," which gives meaning to the action and reaction patterns represented in the 'residues' and 'derivatives.'[88] Most important for "social equilibrium" is thought to be "a proper mixture of residues I and II"; in other words, of innovation and conservation, in terms of which common-sense phraseology his social-equilibrium pronouncement sounds rather platitudinous.[89]

Returning to Handman's analysis, we see history take on, through Pareto's methodology, the aspect of a "succession of efforts to appear logical." With his vague and broadly bifurcate concepts in mind, Pareto combed the vast field of Western European history from 1870 to 1914 and "selected instances and illus-

[86] Mayer, *Ibid.*, p. 101.
[87] *Supra*, p. 444.
[88] "Methods," p. 144.
[89] *Ibid.*, p. 147.

trations on which his notions could be used as instruments."[90] He found uniformities in historical material by seeking for them and locating examples to his liking. With other preconceptions, one might "find things precisely the opposite" and also by means of a similar "logico-experimental" approach.[91] In conclusion Handman speaks of the *Sociologie Générale* as a "monumental piece of work, monumental because of that very passion for a scientific approach which it does not reach and for the terriffic blows it gives to the numerous preconceptions different from its own."[92]

Pareto's work appears as an excellent illustration of the futility of attempting comprehensive studies of social phenomena, without an adequate methodological perspective and essential tools for social research. His investigations were certainly aimed in the right direction. He endeavored to be scientific and to arrive at fundamentals. Such good intentions are important, but they may nevertheless do more harm than good unless the full significance of scientific method is appreciated, the theoretical along with the observational; unless the appropriate place of each significant instrument used in social study is clearly understood; and unless the most important techniques (as the psychological) are put first and subsidiary tools (as the physical and mathematical) are put where they belong. Pareto explored the social field with a one-sided appreciation of scientific method which rendered him purblind to his own presumptions while he criticized like presumptions in others; with apparently no knowledge of psychological procedures, he substituted formalistic mechanical concepts for more significant ones; and he depended for verification upon historical data without understanding the limitations and criteria applying to such data. Like Fay, he dealt with historical material covering the period from 1870 to 1914, but he picked his cases to suit his purposes, whereas Fay handled the

[90] *Ibid.*, p. 151.

[91] *Ibid.*, p. 152. *Cf.*, also, G. M. Graham, "The Logics and the Social Sciences," and W. D. Wallis, "The Problems of an Empirical Sociology," *Social Forces*, September, 1928, pp. 24–32, 46–49.

[92] "Methods," p. 153.

material dispassionately and with full knowledge of current historical methodology.

The other case-book analysis, mentioned at the beginning of this section as exemplifying the combined use of historical and scientific procedures in social study, is that pertaining to the work of Williams on rural attitudes.[93] This is a much less ambitious undertaking than the work of Pareto but, at the same time, probably more significant because of the careful utilization of known criteria. Williams made no pretense at being rigorously scientific, limited his studies to a small geographic area, and attempted a judicious combination of historical and current social-study techniques even though in this he might conceivably have done somewhat better.

Williams' work is frankly preliminary and exploratory. For over twenty-five years he has been in close touch with a small New York town, investigating its present and past population from 1825 on, studying its geographical and economic environment and the 'attitudes' of the people toward such questions as the weather, family and sex relations, pioneer life, social and economic customs, religion and education. From our present-day point of vantage, it is possible to say that more rigorous bio-psychological and geological techniques might be employed, further 'genetic' and racial studies might be undertaken, additional methods of historical criticism might be used. These are important matters of detail. But much more important is the combination of procedures to which Williams' work points, especially his attempt to ascertain the attitudes and beliefs of people now gone, through local documentary sources, the direct observation of the investigator, and the oral testimony of old residents.

The first two of the procedures used by Williams require training in all the historical and scientific techniques reviewed in these pages. The third procedure suggests the need for developing a new social-science instrument, namely, for the taking of oral testimony. What methodology there exists in this respect is still very crude. Psychological aids are naturally indicated and are being employed here and there; so are judicial procedures

[93] *Ibid.*, analysis 32.

indicated, with the court-room atmosphere left out. Here is an almost virgin field for the deviser of techniques, to which we shall return presently. . . .[94]

In the foregoing pages have been reviewed the scientific and historical relationships, techniques, and concepts which are being found useful in social study, whether in preliminary, exploratory, or more rigorous applications. Most of the case-book analyses concern themselves with such methods, forty-four out of fifty-two, thirty-five dealing with natural-science procedures and nine with historical. The relationship of social phenomena to the recognized sciences is taken up in two of the analyses; bio-psychological studies into the fundamentals of human nature, in another two; psychological and behavioristic instruments and techniques, in seven; the 'genetic' approach to an understanding of existing social relations, in five; bio-geological, prehistoric, and environmental approaches, in seven others; mathematical and statistical tools applicable to the social field, in eleven; historical techniques, in eight; and the utilization of combined historical and scientific procedures in another two.

For social study these methods, with the exception of the combination of techniques which should come first rather than last, would seem to be important in about the order indicated, the historical and the mathematico-statistical being used most effectively in conjunction with the more significant social-science procedures mentioned. From among these, and others similar in nature, the student of society will ultimately, it would appear, select and develop those tools and concepts of most assistance in the building of a science of society. In addition, he will create other categories and methods that are distinctively his own. Complex social problems which, in resolution, may lead to further basic concepts or methods are suggested in the eight remaining case-book analyses.

IV. SPECIAL SOCIAL-SCIENCE CONCEPTS AND PROCEDURES

Social causation and law

One of the most significant problems of social theory concerns the question of social causation, which still remains where we left

[94] *Infra*, pp. 471–472.

it with Schevill. Three additional case-book analyses throw interesting side-lights upon this important issue. From the point of view of the modern historian, Fay presents an illuminating review of Bury's study of the concept of progress.[95] The Greeks thought of a "fixed order in the universe," which man could not change. The Middle Ages substituted the idea of arbitrary acts of Providence for the fixed order. With the achievements of modern science, man's increasing control over physical nature brought with it the concept of human progress, to which the doctrine of evolution was at first thought to add corroboration, whereas in fact it appears to have little bearing on it. If there is progress, Bury holds that it must be relative to time and place. But, if relative, adds Fay, another criterion of social judgment may some day displace it. . . .

The point of view of the sociologist and anthropologist is presented in the discussions of Chapin's and Kroeber's study of cultural change, which constitutes the longest analysis in the case book.[96] As a preliminary, the analysts, Allport and Hartman, suggest four possible methodological concepts or levels, useful as approaches to an understanding of various phases of experience, including the cultural. First is the natural-science level, on which through analysis and "explicit denotation" the invariable sequences and predictable happenings of nature are discovered. Second is the 'genetic' level, on which is traced for purposes of explanatory generalization "the history or genetic development of the phenomena in question."[97] Prediction here is less reliable, the analysts state, because of changing conditions and unusual situations. Third is the telic level, on which "the significance of the phenomena to the observer" is shown, what they mean in human life.[98] Here prediction is virtually impossible, for it would amount to invention; "explicit prediction would be the invention itself and hence its own fulfilment."[99] Fourth is the stylistic

[95] "Methods," analysis 20.
[96] *Ibid.*, analysis 22.
[97] *Ibid.*, p. 310.
[98] *Ibid.*, p. 312.
[99] *Ibid.*, p. 313.

level, on which the significance of the phenomena to the observer is limited merely to "formal or incidental changes" in man-made objects, that is, to changes which harmonize with our habits "of form, proportion, and balance."[100] This level Kroeber calls the "super-organic"; and the analysts point out that here "prediction means simply reliance upon a fixed trend of events learned from past experience," each stylistic trend or cycle being a law unto itself, with no necessary significance with respect to other stylistic changes.[101] Culture, the analysts continue, retains its identity only on the last three levels; if reduced to the first one by piecemeal analysis, it ceases to be a cultural object.

With some such series of prediction levels or approaches to experience the present writer is in agreement, but there are certain unhappy designations used by the analysts which should if possible be clarified: One, since organic growth is natural, the 'genetic' would seem to be part of the natural-science approach; hence the first level, which omits the organic, had better be called the 'physical-science level.' Two, since all generalization is explanatory and since this applies to physical as well as to biological science, the qualification should thus not appear to be confined only to the latter, as the case-book analysts attempt to confine it. Three, since 'genetic' has a very definite biological natural-science meaning, it should not be made synonymous with 'historical'. Four, the 'stylistic' is apparently only a special phase of the 'telic'. The present writer therefore suggests the following modified scheme of levels: First, the physical-science level; second, the organic or genetic level; third, the psychological level; fourth, the cultural or telic level (including the 'historical' approach but not the 'genetic'). These modifications in the analysts' methodological scheme of 'levels' or frames of reference, with respect to prediction and cultural change, may be made without affecting their essential arguments, and possibly with some avoidance of confusion.

Having outlined four methodological approaches to the problem of causation, the analysts proceed to measure Chapin's and Kroe-

[100] *Ibid.*, p. 316.
[101] *Loc. cit.*

ber's studies against them. They immediately note that the authors apparently use physical-science principles in their assumptions. The view of the authors of "an orderly and predictable sequence in culture" is thus seen to be taken from that level on which our notions of prediction are primarily derived but on which cultural phenomena cease to have meaning. "One cannot explicitly handle, or react to, cultural phenomena in such a manner as to reveal the way they behave independently of human beings; and one cannot analyze them into parts and describe the laws of action of those parts. As soon as such an attitude is taken, the object loses its cultural significance and we find we are investigating a different phenomenon, and one which tells us nothing about the behavior of the 'cultural' phenomenon."[102]

Regarding the difference between 'laws' and 'prediction' on the physical-science level and on the cultural level, the analysts continue: "It seems that many cultural phenomena are obeying laws of their own, thus limiting the very notion of scientific generalization. A plurality of laws of cultural change, with apparently no explanation beyond these laws themselves, is an unsatisfactory condition."[103] More detailed criticism of some of the authors' assumptions is also presented by the analysts: for example, that a cultural complex is not an 'organism'; that a mechanical invention or an institution is not a unitary thing but rather a number of things depending upon the human purposes being served; that one cannot add a number of subsidiary inventions together since they are essentially heterogeneous and possess no common denominator; that neither the organic analogy nor an assumed societal learning process can explain Chapin's three-stage "societal reaction" pattern; that "superindividual principles" do not determine "cultural cycles" nor the course of social events. Summing up their criticisms, the analysts maintain: "The failure to grasp the fundamental difference between natural-science and cultural material has led students of culture to advance certain hypotheses of doubtful value." Kroeber's "mystical notion of determinism," of superorganic forces or extra-human powers,

[102] *Ibid.*, p. 345.
[103] *Ibid.*, p. 347.

"is quite as hopeless for directing future research as the organic analogies of Chapin."[104] The analysts prefer as working hypotheses with respect to cultural objects and changes, the psychological and biological laws motivating "the human beings who invent and perfect such objects" and who are responsible for cultural development. Whatever special 'laws' or cyclical changes a cultural complex may exhibit "upon its own abstract level," these should not be confused with natural-science laws.

The difference between natural-science 'laws' and cultural 'laws' may be further illustrated by a third of the social-causation analyses. This outlines Bloomfield's and Sapir's concept of phonetic law.[105] As Sapir expresses it: "Phonetic laws are by no means comparable to the laws of physics or chemistry or any other of the natural sciences. They are merely general statements of series of changes characteristic of a given language at a particular time. . . . Such phonetic laws have been worked out in great number for many Indo-European and Semitic languages."[106] Such laws are found to exist, also, for primitive languages. "It may be pointed out in conclusion that the value to social science of such comparative study of languages . . . is that it emphasizes the extraordinary persistence in certain cases of complex *patterns* of cultural behavior regardless of the extreme variability of the content of such patterns. It is in virtue of pattern conservatism that it is often possible to foretell the exact form of a specific cultural phenomenon."[107]

Such preliminary attacks upon the complex problem of social causation, indicate quite clearly that among the primary concepts with which social science must reckon are sufficiently comprehensive definitions of 'causation' and 'law'.[108] Two conceptions of 'law' have already been examined: first, the natural-science idea of a uniform, invariable, uncontrollable, and clearly predicta-

[104] *Ibid.*, pp. 348–349.
[105] *Ibid.*, analysis 21.
[106] *Ibid.*, pp. 297–298.
[107] *Ibid.*, p. 306.
[108] For some of the problems here involved see Morris R. Cohen, "The Social Sciences and the Natural Sciences," a chapter in "The Social Sciences," edited by Ogburn and Goldenweiser, *Houghton-Mifflin*, New York, 1927, pp. 460–465.

ble sequence of physical and biologic events, such as Kepler's law of planetary motion or the biologic law of recapitulation; second, the cultural concept of trends and cycles characteristic of given culture-patterns and institutions at particular places and times, persistent as a habit is persistent, but otherwise variable and unpredictable except insofar as they are deliberately man-made, to which extent they are controllable.

A third notion of 'law' is the legal conception of an ordinance, statute, code, or constitution, written or unwritten, expressive of the social will. This likewise receives consideration in the case-book.[109] Legal concepts, it is there stated, have come to be divided into positive and non-positive aspects: the former thought of as including formal enactments, decrees, decisions, and rulings actually enforceable upon the people; the latter, social customs and traditions not yet formally recognized and enforced. Non-positive 'law' is thus the same as the mores.[110] Time was when both positive and non-positive 'law' were regarded as a part of 'divine' or 'natural' law, conceived in *a priori* fashion. The positivists, represented by Mattern the case-book analyst, disavow any such approach; they hold, rather, that positive 'laws' are deliberately man-made and vary considerably with place and time and with fallible human judgment, and that non-positive 'laws' or mores are "nothing but precedents and conventions established at a time when it was not customary" to write things down.[111] This does not mean to the positivist that the mores are not founded upon a human disposition for justice, honor, and fair play, but merely that any *a priori* assumption of "the unity of the legal order" or of an eternal "international constitution," as presumed by Verdross, is wholly imaginative and out of harmony with modern scientific tendencies in the study of constitutional and international law and of the realities of life.

There are thus three different conceptions of 'law', four prediction levels, and the idea of progress, which studies into social causation and law are bringing to light and which the student of social-science methodology must take into particular account.

[109] "Methods," analysis 6.
[110] *Cf.*, *supra*, p. 443.
[111] "Methods," p. 126.

Precise meanings for science and basic concepts: social work, evidential techniques, politics

Throughout these pages the expressions 'scientific method', 'the sciences', and 'basic variables' have been freely used, and some attempt has been made to indicate broadly what meanings may legitimately be attached to them. A few additional remarks on this score will be helpful here in connection with several of the case-book analyses still to be reviewed.

As stated at the beginning of the article, 'scientific method' is best thought of as a combination of verified observation and theoretical analysis, or, more briefly, as empiricism and rationalism. In this broad sense it is applicable to any realm of experience, including the social, although the proper balance between its dual aspects is not always maintained.[112] As for the meaning of 'science', it is frequently assumed that any field to which the scientific method is applied becomes, *eo ipso*, a science. Such a deduction is without serious meaning. In the first place, to achieve the said result, the scientific method must be very rigorously applied.[113] In the second place, this strict methodology

[112] *Cf.*, Pareto's point of view, *supra*, pp. 458–459; also, Mayer, "Toward a Science of Society," pp. 163–165.

[113] *Cf.*, Jerome Michael and Mortimer J. Adler, "Crime, Law, and Social Science," *Harcourt Brace*, New York, 1933, pp. 56–63; Mayer (*op. cit.*, p. 163) summarizes this strict application thus: "On the empirical side, a science must go beyond mere information or description based upon individual observation or common-sense knowledge alone; the observations must be accurate and reliable, they must be tested repeatedly by competent observers, must cover broad and significant categories of events rather than particular things or mere aggregates of particular things, must be supplemented by experimental, statistical, or other special observational techniques, and must be directed by an adequate 'theory or analysis.' On the rational side, a science, in its propositions or generalizations, must transcend mere opinion or common belief; its propositions must have a generality which is broader than the empirical evidence from which they are drawn, they must nevertheless rest upon such evidence and be validated by it, must possess the formal character of expressing a relation between variables, and must be members of 'compendent sets of propositions.' An empirical science may be distinguished from a purely rational discipline as follows: The one rests upon an observational as opposed to a rational base, whereas the other is built upon a set of arbitrary definitions, axioms, and postulates; the propositions of the one are compendent but not systematic, while the other builds upon its rational base a dependent group of theorems which are thoroughly systematized; the empirical hypotheses of the one are validated by the process of induction, i.e., by

must be logically adequate to fundamental problems of human experience, otherwise 'sciences' of ten-cent cigars, pewter soldiers, or mud pies are just as reasonable as any others.[114] In the third place, 'fundamental problems' not yet comprehended within the fields of the established empirical sciences (physics, astronomy, chemistry, geology, and biology) must be 'compendent' with, or structurally and 'genetically' related to, those fundamentals already recognized.[115] Out of such new problems, when so related and constituted, there may develop 'basic concepts' of new empirical sciences, as is already thought by some to have happened in psychology, and as Comte expected would some day happen in sociology.[116] With these considerations in mind, the remaining case-book analyses may be the more readily appraised.

As to whether or not modern social case-work is a science or is about to become one, a contention advanced in one of the analyses, little need be added to what has just been said.[117] That the word 'science' is often loosely employed, and that medicine is sometimes referred to as a science, is of course common knowledge, but that such lay usages should be regarded as proper bases for serious analysis is an entirely different matter.[118] As for social case-records and concepts, they have in recent years assuredly become much more comprehensive and adequate with respect to meeting the practical problems of personal maladjustment in "pacific Western society;"[119] but the professional social worker is apparently still falling far short of making fundamental contributions to an understanding of the basic variables of human nature and culture. One looks in vain among the items of "current personal

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reference to further observational evidence; the rational theorems of the other are proved by deductive logic solely. To inductive proof in empirical science the adjective 'probable' applies: to the purely deductive proof of a rational discipline the adjective 'true' is appropriate."

[114] *Cf.*, "Methods," p. 703, with Sorokin, "Contemporary Sociological Theories," pp. 80–81.

[115] *Cf. supra*, pp. 432–435.

[116] The foregoing discussion has no bearing upon so-called "applied sciences," such as engineering.

[117] "Methods," analysis 4.

[118] *Ibid.*, pp. 96–97.

[119] *Ibid.*, pp. 98–108.

data" records[120] for anything even remotely resembling thoroughgoing physical and mental examinations and psychological tests to determine the relative strength of prepotent drives or of other fundamentals characterizing maladjusted individuals.

These observations pertain primarily to the points of view of average professional social workers, to the results of their recurrent conferences on social work, and to the personal comments of the case-book analyst, Philip Klein, who essays to review Miss Richmond's important volume, *Social Diagnosis*.

This book marks distinct advances in several directions. One is her effort to find a unifying concept, in place of such terms as 'sick', 'poor', 'delinquent', 'feeble-minded', 'unemployed', for the designation of any type of socially maladjusted or unadjusted individual, although the combination of her selected legal notion of 'client' with a medical analogy is somewhat unfortunate and a better designation will doubtless be found. Another and still more important advance is her painstaking attempt to codify present practices among social workers with respect to "the collection of evidence and the drawing of inferences therefrom" and her endeavor to point the way to improvements.[121] Here her work treats of such subjects as the nature of evidence, the processes of obtaining evidence, sources of evidence, variations or special conditions determining the selection of evidence, the difference between real, testimonial, and circumstantial evidence, the competence and bias of witnesses, and the like,—a truly notable beginning of what, in the opinion of the present writer, should some day become as much a disciplined set of procedures as is the historical or the statistical.

To the development of rigorous evidential techniques, the professional social worker can surely contribute, especially on the side of the taking of oral testimony, if, in the spirit of Miss Richmond's work, he will learn to look beyond his immediate pragmatic task to sociological fundamentals. Other specialized disciplines will likewise need to lend a hand, and in a similar spirit.

[120] *Ibid.*, pp. 99–100.

[121] Mary E. Richmond, "Social Diagnosis," *Russell Sage Foundation*, New York, 1917, p. 28.

Social evidence has its legal side, its judicial side, its historical side, its psychological side, its political side, illustrated in one way or another in almost any worth-while social investigation that might be named.

The case-book analyses show the need for reliable evidential procedures in a number of instances, as excellent an example as any being the work of Bryce in the field of politics.[122] Bryce long participated actively in public affairs and he well knew how to make use of documentary evidence, both of which procedures he specified as important aids for political understanding. He did not mention, however, "the technique in which he so remarkably excelled, the technique of interrogation and of field observation."[122a] "It may well be asked," wrote a competent critic of the writings of Bryce, "whether since the days of antiquity there has been any important historical work written so largely from the talk of living men."[123] Bryce's failure to mention the fact-gathering instrument upon which he relied so extensively and in the use of which he was such a past master, is significant as indicating that in 1921, when he published his *Modern Democracies*, evidential procedures in politics were still largely intuitive. Bryce was a cautious writer and extremely self-critical, which, with his wide experience and intimate knowledge, probably accounts for the scholarly vogue his publications have secured. He was pessimistic about politics ever becoming a science but thought his methods were sufficiently strict for a pragmatic discipline. Nevertheless, his definitions are ambiguous, the criteria he used remain without "theoretical justification," and his terminology "has often been criticized as more confusing than enlightening."[124] Lasswell, who analyzed Bryce's work in the case-book, disagrees with the view that political terminology and technique are already about as scientific as one can hope for. To a further consideration of these questions, in another of the case-book analyses, we may now turn.

[122] "Methods," analysis 33.
[122a] *Ibid.*, p. 468.
[123] *Ibid.*, p. 470, quoted from H. A. L. Fisher.
[124] *Ibid.*, pp. 474–478.

The next analysis is entitled "The Possibility of a Science of Politics,"[125] and it in an able manner brings to the fore additional issues bearing upon the meaning of 'science' and 'basic variables', applicable not only to the field of politics but to all social disciplines. At the outset two factors are pointed to, the historical and the normative, which distinguish politics and the rest of the social studies from the physical and biological sciences and which are usually presented as grounds for denying the possibility of turning social study into social science: history deals with particular and unrepeatable events whereas the sciences deal with universals; normative elements are subjective valuations which "cannot be measured or demonstrated."[126] In respect to these assertions the present writer would say, first, that insofar as social study depends primarily upon history it cannot become in any true sense scientific, but that insofar as its primary dependence is upon current social manifestations of the basic concepts of human nature, and historical data are regarded as wholly subsidiary, there is no apparent reason why it cannot be organized into a science; secondly, that there is no real occasion for pessimism about the 'measurability' or 'demonstrability' of human values, since modern psychological techniques, as already indicated, are taking these matters definitely in hand, although the results are still meagre and inadequate.

The analyst, W. Y. Elliott, presents the views of two opposing groups among contemporary students of political phenomena,—that of the political philosophers, who regard politics as an art rather than a science, and that of the political fact-gatherers, who follow Baconian methods and fancy they can fashion a science of politics upon physical-science precepts. He gives an excellent criticism of the use of mechanical and biological analogies in politics, such as theories of the pendulum, of the cyclical degeneration of governments, and of the 'organic' birth and death of civilizations; and, up to a certain point, his analysis of the meaning of 'science' is likewise penetrating. It is quite true that in the sense of "an objective description of observable and describable

[125] *Ibid.*, analysis 3.
[126] *Ibid.*, p. 70.

external characteristics of human behavior, there are at least as good grounds for calling politics a science" as there are for calling any of the other social studies sciences.[127] But, as suggested above, such grounds, even though rigorously conceived, are not enough; there must be logical adequacy and a commerce with essential variables. On these further grounds, the most important immediate question is whether the problems with which politics now concerns itself are fundamental in human nature and culture, not whether they are merely "observable and describable" behavior characteristics of any variety.

Elliott's conception of politics, as a combination of fact-gathering and philosophy strictly construed, would seem to provide an excellent illustration of the best that may be said for any of the specialized social studies as at present constituted, that is, before the basic categories of human nature and of social relations have been definitely formulated. Politics, he holds, "does not submit itself to rigid deterministic laws because men are purposive and morally creative beings"; in this field, prediction and experimental verification are not to be anticipated; as a mere classificatory and descriptive discipline, "it can hardly go beyond the state of botany in the days of Linnaeus"; it must formulate pragmatic units for investigation, as the 'state', certain political 'pressure groups', 'political myths', etc.; "the results will be too complex to permit statement as *laws* in any way analogous to the laws of physics"; but they will at least provide "interesting leads," if not "complete scientific analyses." In short, politics is an attempt "to understand the whole complex of forces that affect government by men under institutions, laws, and organized force," to study "the behavior of groups when the groups have become highly institutionalized," "to evaluate the ends of government" in terms of "an ideal scale of values," to classify political movements, and to arrive at "rough laws." Politics would thus seem to be an art and an applied discipline of a pragmatic nature, not to be grouped with the recognized sciences. Nor should the other specialized social studies be so grouped.[128]

[127] *Ibid.*, p. 80.

[128] *Ibid.*, pp. 79–87.

There is one thing that Elliott indicates cannot be done, namely to abstract a "political man out of the total context of human activity," as Catlin attempts to do in his effort to match the 'economic man' of the classical economists. Such methods are now "thoroughly suspect."[129] Human nature is simply not composed of water-tight compartments made up of a political man, an economic man, a family man, a legal man, an historical man, an ethical man, and other varieties, all packed together like a juggler's nest of boxes. The primal dispositions of men cut across such fictitious categories, and a true scientific understanding of social relations will likewise cut across them.

It would thus seem evident that those interested in formulating a social science will need first of all to employ definitions of science and of basic concepts which conform to the meanings assigned to these terms in the recognized sciences; then they will need to formulate basic social categories in accord with these definitions. Such basic categories, when found, will be among their most important methodological tools. Another indispensable social-science instrument appears to be an evidential technique competent to serve as a scientific observational procedure for the collection of social facts.

Social preconceptions: economic utility and value

There are today innumerable misconceptions in social study to hamper progress. This is not the place to attempt any exhaustive catalogue of them;[130] but as they are revealed in the case-book analyses we are concerned with them here.

The history of science suggests that it may be helpful to draw a pragmatic distinction between misconceptions and preconceptions. Among the former might be listed certain current ideas which represent exaggerations, wrong emphases, or crude patterns of thought, reflecting a prevailing mode that soon disappears and carries away with it most of the dependent misconceptions. Such probably are the current mechanistic and behavioristic emphases

[129] *Ibid.*, pp. 82–83.

[130] *Cf.*, Sorokin, "Contemporary Sociological Theories;" Mayer, "Toward a Science of Society," pp. 171–172; Mayer, "Seven Seals of Science," pp. 389–430.

in social study, or, possibly, the 'residues' and 'derivatives' of Pareto. Misconceptions, thus viewed, more or less dissipate themselves. Among the preconceptions, on the other hand, might be listed those persisting misconceptions that survive at any given period. Having as a rule received the stamp of autnoritative approval when projected, these by a process of subtle sophistry are reformulated and modified to meet recurring criticisms, until succeeding definitions become progressively vague and ambiguous; and thus the errors continue to be accepted largely through reiteration and force of custom. These established preconceptions are the greater stumbling blocks to constructive advance, and to remove them has sometimes required centuries of effort. Among them the present writer would place political and economic 'men.'

Probably in no specialized social field have preconceptions taken a deeper hold than in economics, doubtless because this discipline became systematized somewhat earlier than others and because the prevailing notions at the time—hedonism, forces in mechanical equilibrium, homogeneous units of measurement—were admirably adapted to place a money calculus and human motives on the same apparent plane. The cornerstone of this economic structure of preconceptions, surviving to the present day despite its now clearly fallacious psychological and physical-science underpinning, is the classical theory of economic value, with its subsidiary notions of utility and disutility. These dogmas are touched upon in one of the case-book analyses still to be examined and in one other analysis previously taken up in a different connection. The question of 'value' has confronted us constantly in these pages, so that it may be helpful to give more than passing attention to it here.[131]

The remaining analysis is by Frank Knight on the work of Jevons and others.[132] Knight's discussion concerns itself largely with certain relatively-recent misconceptions in economic theory —physical-science and behavioristic assumptions already touched

[131] *Cf.*, Statement by Elliott, "Methods," p. 91, that Politics must deal "with values in an ideal scale."

[132] "Methods," analysis 2.

upon a number of times in preceding pages.[133] He canvasses three notions which are often confused in current value discussions. First is the assumption that physical science deals entirely with readily observable facts and that subjective implications are nowhere in evidence. Yet, as he points out, many physical operations (such as molecular movements) are not directly observable, and hypothetical concepts and generalizations (such as the ether, radiant energy, and spatial curvature) are widely used. "In the twentieth century," he adds, "it is not the physicist but psychologists and sociologists who accept a physical interpretation of nature." Second is the false idea that it is possible to build up a factual economics without explanatory features, as is being attempted by extremists in the commodity-statistics, index-numbers, and price-economics movements. Commodities, he maintains, should not be conceived in terms of physical entities but in terms of the human services or satisfactions they render. "There is no such thing," furthermore, "as an objective average of prices," for subjective implications cannot be left out. It is the *meaning* that prices have for human beings and the *motives or purposes* that lie behind human behavior that are the important considerations. Third is the assumption by many (who would be in agreement with Knight's criticisms up to this point) that in social study, as in natural science, one can avoid ethical or normative implications. Explanation, Knight holds, is not enough, for practical policies and desirable human ends cannot be escaped in social theory; and this fact means standards of ethical comparison and evaluation.

With Knight's penetrating criticism of these three current misconceptions in economic theory, the point of view presented in the preceding pages is in complete accord. Where this criticism, however, is used by Knight as an ostensible offensive to cover up or to bolster an actual defence of classical preconceptions concerning the notion of economic utility (proceeding largely after the manner of strong reiteration and occasional *non-sequiturs*),

[133] *Cf.*, also, discussion regarding subjective-objective relation by Mayer, "Scientific Method and Social Science."

it is quite out of harmony with that point of view and in fact is in complete disagreement with it.

As for the actual meaning of economic utility, very little is vouchsafed in his analysis. We are told: (1) that utility is " 'that which' men act in order to get and to maximize"; (2) that it is "simply the universal principle of motivation quantitatively conceived" (universal in anticipation though not always actually realized in fact); (3) that whether it is to be identified with pleasure "is an issue properly outside the field of economics"; (4) that it results in "satisfaction"; (5) that it is an explanatory concept "more or less closely analogous to force in physics" and thus may be used "to explain behavior"; (6) that the idea of real value "most obstinately adheres" to it. The first version of the concept thus defined informs us that utility is an objective end; the second, that it is a subjective motive, in some way quantitative; the third suggests that as an end it might be "pleasure"; the fourth, that the end is "satisfaction"; the fifth version renders it a subjective motive once again; and the sixth is uncertain in this respect but suggests qualitative rather than quantitative distinctions. We gather, then, that the concept in question is both subjective means and objective end, that it is quantitatively measurable, yet serves as a qualitative norm or standard, and that whether viewed as means or end it may be identified with 'satisfaction' and possibly with 'pleasure.'

Other general ideas will also fit this hazy definition. If pleasure or satisfaction is to be regarded as a motivating force, it must be in some vague anticipatory sense. In this sense 'life' (or 'the will to live') and 'money' likewise conform to the definition. Life or money may be thought of as 'that which' men wish to maximize; the will to live or money becomes, in contemplation, a universal motivating force; it results in satisfaction, although (as with utility) not always; it explains human behavior; it has value most obstinately adhering to it; and it is quantitative as to time or amount. Possibly 'money' fits the definition best of all. The first count, therefore, against the concept in question is its vagueness and ambiguity.

If construed as economic utility, a second count against the

concept is that, in any realistic sense, it cannot be quantitatively homogeneous in character, a fact clearly indicated by C. E. Persons in an historic analysis.[134] A third count is that it continues to be based upon an out-moded psychology, incurably hedonistic and rationalistic, whereas instinctive drives, dispositions, interests, or desires (which McDougall and Thomas-Zuaniecki found to be the motivating forces behind human action) are for the most part impulsively irrational.[135] A fourth count is that, as actually employed in economic reasoning, the concept 'utility' carries with it at least two of the three current misconceptions against which Knight contends. In short, in his analysis economic and broader human valuations are confused.

This confusion is admirably portrayed and clarified by Hobson in his writings.[136] In economics, Hobson points out, 'utility' attaches itself to consumable goods of every variety, that is, to the gratification of economic wants; humanistically, it means vital usefulness exclusively, or that which makes for human welfare. In economics, 'cost' or 'disutility' merely signifies 'productive' effort of any kind; humanistically, it connotes anything that militates against human welfare. Thus, in economics, 'utility' attaches to all consumption; 'disutility', to all production; whereas, humanistically (in making for human welfare, pleasure, or satisfaction), no such line of demarcation can be drawn. As for consumption, three important factors are to be considered: organic needs, commercial pressure, social prestige. Only those goods and services satisfying organic needs should be regarded as always possessing human utility. Where commercial pressure through misleading advertising and where a craze for social position result in harmful or distorted cravings and conspicuous

[134] "Marginal Utility and Marginal Disutility as Ultimate Standards of Value," *Quarterly Journal of Economics*, August, 1913, pp. 547–578.

[135] *Cf.*, Joseph Mayer, "Pseudo-Scientific Method in Economics," *Econometrica*, October, 1933, pp. 418–430 (426–427n).

[136] "Methods," analysis 37. *Cf.*, *supra*, p. 446; also "Conventional Economics and Human Valuation," H. C. Overstreet, *Journal of Phil. Psych. and Sc. Method*, Vol. 12, 1915, pp. 281–282; Walton H. Hamilton, "Economic Theory and Social Reform," *Journal of Political Economy*, Vol. 23, 1915, pp. 562–584; Paul T. Homan, "Contemporary Economic Thought," *Harpers*, New York, 1928, pp. 281–374.

waste, human disutility is the outcome. As for production, the work of the scholar, scientist, or artist has much of pleasurable satisfaction or human utility in it; only at the lower end of the scale of routine work, where there is fatigue and monotony, do we find human cost or disutility. Similarly with saving or 'waiting': the rich save automatically; the middle-class, with forethought and self-control; the poor, with difficulty and sacrifice. Solely with the poor should saving or waiting be regarded as colored with disutility. A humanized body of economic doctrine would, therefore, be based entirely upon fundamental human needs, regarding the satisfaction of these alone as possessing 'utility,' all else as possessing 'disutility.' Thus the principle of maximizing utility and minimizing disutility or cost would seem to have a clear-cut meaning only in terms of human welfare. (Maximizing production and minimizing consumption simply does not make sense.) By the same token, in economics, 'value' is taken to mean merely "the power of a good to command other goods in exchange," so that, if a book and a burglar's jimmy each cost a dollar, they are equally 'valuable'; whereas humanistically, the book is evidently greater in value. Thus the concept 'value' might better be viewed, says Hobson, as a balance of human utilities against human costs, in terms of the best ordering of human interests and welfare.

This, in brief, is Hobson's appraisal of utility and value, economically and humanistically conceived. In its light, Knight's arguments take on added implications. If, as Knight holds, the actual motives and purposes behind human behavior and their ethical and normative connotations are important in economics as elsewhere in social study, then he must apparently cease viewing 'utility' in classical economic terms exclusively, for his definition of 'utility' is broad enough to include the humanistic interpretation. But should he agree to this broader view, the prelude of his analysis and the arguments of 'marginal utility' theory which it implies, would no longer seem to have meaning.[137]

Surely, as Knight says, "some terminology should be adopted"

[137] "Methods," p. 59: The argument on this page in the case book construes 'utility' in formalistic economic terms; for the rest of Knight's analysis, the interpretation of the concept is chiefly humanistic.

in social study which will truly represent "the actual desires of individuals."[138] The present writer has already indicated in preceding pages the steps to be taken: the concepts formulated must reflect human-nature fundamentals; they must be rigorously conceived; they must be logically adequate; and they must be 'genetically' and structurally related to the primary variables of those sciences, psychology and biology, which are nearest of kin to social activity. No deceptive, handed-down imponderable, encrusted with hoary misconceptions and super-refined until like the chameleon it takes on colorings to suit the occasion, can serve the purpose of a basic social category. To the present writer, the classical dogmas surrounding the ideas of economic utility and value are among the most stifling preconceptions with which students of social phenomena have had to contend.[139] The three current misconceptions reviewed by Knight are as nothing compared with these older ones and merely represent a reaction with respect to them. If it was important to clear away ancient effluvium in the study of electricity and magnetism, it is at least as important to clear it away in social study,—a task of no mean dimensions considering the depth of most social prejudices.

Summary and conclusions regarding social-science methodology

Our review of the case-book studies is completed. What they indicate with respect to the most-approved present-day methods and concepts used by students of social phenomena, may now be epitomized, as follows:

(a) Social science must be structurally related to the already-recognized sciences as Comte indicated, especially to psychology and biology, which are nearest of kin; and until the basic variables of human nature are established, social study is bound to be enmeshed by a "sterile and encumbering mass of irrational special discussions." The case-book analyses show that many of the social investigations now being carried on are still of a highly preliminary, superficial, or exploratory character.

[138] *Ibid.*, p. 69.

[139] *Cf.*, Jacob H. Hollander, "Economic Theorizing and Scientific Progress," *American Economic Review*, March, 1916, pp. 124–139. Hollander's criticism, written nineteen years ago, would seem to hold about as much now as it did then.

(b) Psychology and biology are systematically endeavoring to provide the necessary fundamentals for social study, as in the work of McDougall on primal instincts, and are fashioning various tools and techniques of particular usefulness, especially the self-observational procedures for studying higher mental processes being worked out in the Würzburg laboratory and by the followers of Ebbinghaus.

(c) 'Genetic' studies of existing social relations are also important, as in Sumner's work on folkways and mores, the investigations of Thomas and Znaniecki on the social attitudes of Polish immigrants, and the studies of Malinowski and Boas on family, tribal, racial, and folk-lore antecedants.

(d) Biological and geological methods are likewise proving of assistance in reconstructing prehistoric cultures and indicating the conditioning rôle of natural environment, as in stratigraphic and archeological researches, in Wissler's studies of the cultures of primitive peoples, and in Ezekiel's work in agricultural economics.

(e) Mathematico-statistical techniques are being found increasingly helpful in the handling of social facts, as in Moore's work on the demand function and in the widespread use of statistical methods, although these sometimes tend to be regarded as an end rather than a means. Mitchell's study of business cycles affords probably the best example of the effective application of statistical aids.

(f) Besides the social-science techniques developed by the scientific disciplines, historical procedures are important (as in documentary criticism and criteria of authenticity and credibility) for the resurrection and proper understanding of what vestiges of past social events are still existent. Pirenne and Schevill provide exceptionally illuminating analyses in this connection.

(g) Unless a broad methodological perspective in social study is maintained and scientific and historical procedures are properly combined, the best-intentioned investigator, as, for example, Pareto, is likely to go astray.

(h) In addition to utilizing instruments and categories developed by history and the recognized sciences, the student of society is

developing specialized concepts and methods of his own, as in the idea of progress, in various significances discovered with respect to the notions of law and levels of prediction, in more inclusive and rigorous meanings for science and basic variables, and in the appreciation of the need for developing a rigorous evidential technique.

(i) Inherited preconceptions imbued with sophistry, such as the notions of economic and political 'men' and the idea of economic utility, must be swept away in social study just as similar preconceptions have been swept away in the recognized sciences.

At the beginning of this article a brief outline was presented of what the history of the recognized sciences demonstrates as necessary steps to be taken in the organization of a new science. With the material in the case-book rearranged as indicated, and evaluated in the light of the considerations offered by the history of science, there appears to be every reason to believe that present-day methodology in the social studies is developing in a consistent and constructive manner and that Comte's prediction of a science of society will be ultimately fulfilled. Important next steps would seem to be: to determine upon the primary categories of human nature and social relations; to formulate adequate evidential procedures to serve as necessary observational techniques for social study; to achieve a proper methodological perspective in whatever researches are undertaken; and to eliminate existing preconceptions that hamper progress.[140]

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[140] The point of view presented in this article may be compared with the methodological classifications outlined in the following references: E. W. Burgess, "The Trend of Sociological Research," *Journal of Applied Sociology*, Jan.–Feb., 1924, pp. 131–140; C. A. Ellwood, "Scientific Methods of Studying Human Society," *Journal of Social Forces*, March, 1924, pp. 328–332; Floyd N. House, "General Methodology," Publications of the American Sociological Society, Vol. XXI, 1927, pp. 171–172; W. M. Kozlouski, "The Logic of Sociology," *American Journal of Sociology*, May, 1928, pp. 914–915; Geo. A. Lundberg, "Trends in American Sociology," *Harpers*, New York, 1929, pp. 408–416. For a history of methodology in social study, see L. L. Bernard, "The Development of Methods in Sociology," *The Monist*, April, 1928, pp. 292–320.

Law as a Social Science

BY

HUNTINGTON CAIRNS

IT IS the contemporary belief, in American legal circles at all events, that law or jurisprudence, whatever it may have been in the past, has now the status of a social science. This is an assumption easier to make than to substantiate, and in view of the increasing insistence upon this point, it is now appropriate to inquire whether or not it possesses a tangible foundation. This requires a consideration of the distinctive characteristics of social science, the determination whether or not jurisprudence exhibits these characteristics, and, if not, if it is possible for it to assume them. It will be convenient to begin, briefly and concisely, with an examination of the characteristics of modern legal thought.

Legal thinking is no exception to the general rule that the distinctive characteristics, even the choice of subject matter, of many domains of thought, are determined by their first assumptions. Maine's[1] conviction that civilization is a rare exception in world history, and that Rousseau's doctrine of natural rights was inimical to its continued existence fixed the general character of his theory of law and government and influenced the selection of the problems he chose to investigate. Brooks Adams'[2] materialist conception of history led him inevitably to the conclusion

[1] Ancient Law (World's Classics ed. 1931) 18; Popular Government (1886) 75, 134, 143, 152–54, and passim.

[2] Centralization and the Law (1906) 63–4; The Modern Conception of Animus (1907) 19 Green Bag 12.

that the law's content is the product of the self-interest of successive dominant classes. So it is with other legal thinkers and with other schools of legal thought. Fundamental opinions in law and the social sciences, however the case may be with some at least of the natural sciences, possess a special importance and it is well to pause from time to time to take stock of them.

Underlying the investigations of modern legal research is the conviction that such research can be made beneficial to the administration of justice. It is equally an assumption of the sociological jurists,[3] the realists[4] and the new born experimental jurists.[5] Perhaps this conviction came into legal thought partly as a result of the contempt in which jurisprudence was held by the legal profession of the nineteenth century. "Jurisprudence," said Dicey in an often quoted remark, "stinks in the nostrils of the practising barrister." It would be an adequate answer to the practising barrister, however, and one which would effectively silence him, if it were pointed out that legal research—not in the sense of mere historical investigation, but in the sense of a complete analysis of "law in action"—was being undertaken for the purposes of law reform and not with the aim of inventing another game similar to the entertaining one with which the analytical jurists amused themselves a generation or two ago. Perhaps also the conviction became a part of legal thought as the outcome of the endless stream of criticism directed by social scientists at the actual administration of justice. There seemed indeed to be a prevalent opinion that law could never become a social science unless something were done with respect to the improvement of the administration of justice, although it never occurred to anyone to suggest that economics could never attain the status of a social science unless it corrected the much worse state of affairs obtaining in its field. Perhaps this underlying assumption can also be accounted

[3] Pound, Interpretations of Legal History (1923) 151–165; idem, Criminal Justice in America (1930) 211–12; idem, The Spirit of the Common Law (1921) 212–16; idem, *art. Jurisprudence* in Gee (ed) Research in the Social Sciences (1929) 181.

[4] Llewellyn, *The Conditions for and the Aims and Methods of Legal Research*, American Law School Review, March, 1930, 672.

[5] Beutel, Some Implications of Experimental Jurisprudence (1934) 48 Harv. L. R. 169.

for by the simple fact that legal students here recognized a profitable field of research. Whatever may be its explanation, it is indubitably an essential part of contemporary legal thinking and it has had an unequivocal effect upon the aims and methods of contemporary legal research.

It has had its deepest influence in the selection of the subject matter for investigation. This subject matter in its broadest terms may be denominated "law in action." Each school approaches this field from the standpoint of its own special conceptions but its program rarely exceeds the limits marked out by the subject matter "law in action." The programme of the sociological school, formulated by Pound,[6] insists upon eight points: (1) Study of the actual social effects of legal institutions, legal precepts, and legal doctrines; (2) Sociological study in preparation for law-making; (3) Study of the means of making legal precepts effective in action; (4) Study of juridical action; (5) A sociological legal history; (6) Recognition of the importance of individualized application of legal precepts—of reasonable and just solutions of individual cases; (7) In English-speaking countries, a Ministry of Justice; and (8) That the end of juristic study, toward which the foregoing are but some of the means, is to make effort more effective in achieving the purposes of law. Llewellyn[7] has similarly, but less systematically, marked out for the realists their domain of study. He thinks that the immediate aim of legal research ought to be, and probably for the next ten years will be, the "checking up on the effects of the law in life. . . . first, for the purpose of discovering what the rules really are and mean; second, for the purpose of utilizing them and planning their utilization; third, for the purpose of criticizing them and preparing a way for law reform; and, lastly, as the indispensable basis of any pure legal science which deserves the name." An ambitious programme for the so-called experimental jurists has been stated recently by Beutel.[8] This school's primary object is

[6] Outlines of Lectures on Jurisprudence (4th ed. 1928) 16–18.

[7] Op. cit. supra note 4 at 674; cf. idem, *A Realistic Jurisprudence—The Next Step* (1930) 30 Col. Law Rev. 431.

[8] Op. cit. supra note 5. Cf. Robinson, *Law—An Unscientific Science* (1934) 44 Yale Law Journal 235, for a statement by an eminent psychologist of a legal programme from a psychological point of view.

to discover, with the assistance of an instrument—the experimental method—which they hope will yield more precise results than those obtained in the past, the extent to which law reform is necessary and how it should be accomplished. The programmes of all these schools have in common fundamentally the same subject matter—law in action; and it is apparent that the selection of that subject matter in the first instance was determined in large part by the initial assumption that legal research might be profitable for the administration of justice. Such are the two distinctive characteristics of contemporary legal thought.

A rationalization and defense of these characteristics has recently been put forward by Professor Yntema,[9] who declares that the objective analysis of the legal system for the purpose of reform constitutes the "Copernican discovery of modern legal science." His purpose is to show the scientific legitimacy of the subject matter, the necessity for such an objective investigation, and the probability of the realization of fruitful results. He concludes "that such a study of law in action is an essential, if not under present conditions the most essential, object of a legal science which is not to remain esoteric."

It requires little reflection, however, to realize that the characteristics exhibited by modern legal study are not characteristics of the principal social sciences. Contemporary legal study is a technology; but the social sciences are not technologies. Their ideal, like the ideal of those departments of knowledge commonly denominated the "natural sciences," is the discovery of general laws which unite a number of particular facts. We may pass over the circumstance that the successes of the social sciences in formulating laws have been relatively few in number. The ideal remains warranted in the absence of a demonstration—which is still to be offered—that its realization is impossible. Moreover, the fact that the social sciences have put forward some "general laws" which appear to be "true" with respect to the subject matter for which they were framed, is a positive indication that the ideal is permissible. The ideals of the technologies are, however, entirely different and vary from one technology to another. In the case of modern legal study, it is, as we have

[9] *Legal Science and Reform* (1934) 34 Col. L. R. 207.

seen, law reform. It seems scarcely necessary to point out that this difference in ideals is a crucial one. Each ideal determines in large part the subject matter to be selected for examination, the methods to be adopted, even the facts which will be chosen for study. The possibility of legal study as a technology, moreover, raises a special question, one not peculiar to the other studies we know as technologies. It is the characteristic of a technology that it is indebted to many sciences, although it has in many cases its own contribution to make of matter and method. Mechanical engineering, which is beholden to a score or more of sources, is such a subject. This, however, although characteristic, is not always the case; psychology, which until recently was principally concerned with the gathering of facts as a basis for the formulation of general principles or "laws," has now felt that enough of such principles have been formulated to permit it to become an "applied" science or technology. In its case, psychology is the principal source upon which psychologists draw in making their application. The important point with respect to every technology is, however, that it involves to an exclusive or considerable degree the application of principles or laws formulated by a "pure" science. But this is not the situation in which modern legal study finds itself. It is attempting to be an applied science, although there has been as yet no pure science of law in the sense of a study of the principles governing the relations of law and society. In the absence of such a previous study, the applications of modern legal research must necessarily be of a crude order. They would seem to be confined largely to such problems as the discovery of the manner in which legal procedure should be altered so as to make possible a more expeditious, and at the same time equitable, trial of issues. By its emphasis upon law reform, modern legal study overlooks the fact that it is limiting itself to a circumscribed field of study, and that no matter how successful its accomplishments in this field, it is not a social science nor will it, so long as it maintains its present ideal, ever become one. More important than these considerations, however, is the fact that modern legal study, by reason of its technological ideal, is overlooking a field of investigation which must be tilled before we can ever have a really

fruitful applied legal science. It is the domain which legal research would necessarily explore if it were properly a social science.

The concept of law as a social science implies, for the present at any rate, at least three propositions: (1) That its ideal will be the ideal of the other social sciences, namely, the formulation of statements asserting invariant, or almost invariant, relationships among the facts in its specific field and, in its special case, the organization of such principles into a coherent pattern in conjunction with a rational system of ethics; (2) that its subject matter will be the subject matter of the other social sciences, namely, culture in the sense in which it is understood by anthropologists and the influences which work upon culture;[10] (3) that its methods in general will be the methods commonly employed in the other social sciences.

It should be said at once that legal research so conceived holds out no prospect of ultimate usefulness. Its justification, if it stands in need of one, is not different from that of the other sciences; it gratifies our intellectual appetites. It is not impossible that such research will lead to fruitful ends, but it should be made abundantly clear that no such claims are advanced on its behalf. In this attitude it is no more than assuming the position of the more successful sciences. "Social science," Professor Cohen[11] has written, "can . . . in the long run best attain its goal only when those who cultivate it care more for the scientific game itself and for the meticulous adherence to its rules of evidence than for any of the uses to which their discoveries can be put." It is, indeed, as Titchener[12] observed more than a score of years ago, a distinctive feature of science that it is more rigorous than technology in its observation of the established laws and approved method of logic;

[10] Culture was defined by Tylor as "that complex whole which includes knowledge, belief, art, morals, law, custom, and any other capabilities and habits acquired by man as a member of society." 1 Primitive Culture (1903) 1. A modern definition has been put forward by Malinowski which stresses the fact that artifacts are a part of culture, a point not emphasized by Tylor: "Culture comprises inherited artifacts, goods, technical processes, ideas, habits and values." 4 Ency. Social Sciences, s. v. *Culture* 621 (1931).

[11] Reason and Nature (1931) 349.

[12] *Psychology: Science or Technology* (1914) 84 The Popular Science Monthly 39.

"not," as he was careful to add, "through any superior virtue in the man of science, but simply because the technologist, in the nature of the case, is a logical opportunist, working for results and towards a practical end, and therefore content to work in a logical twilight so long as results are forthcoming and progress can be reported."

Culture as the subject matter of legal research raises a set of problems entirely different from those encountered if the subject matter is regarded as "law in action." Law in action, it should be pointed out, is also within the domain of legal research if culture is taken as its subject matter. Law in action is one aspect of culture, and, as such, an element of the social process which must be studied if the relationship of law and society is to be understood in its entirety. Even here, however, the difference in attitude between the investigation of "law in action" as such, and the study of "law in action" in order to relate it to the social process generally, is important. Facts, as Cook[13] has recently emphasized, do not speak for themselves. Their meaning depends to a greater or lesser extent upon the point of view with which we approach them. "The heavens declare the glory of God to one whose mind is kindled with religious zeal," writes Cunningham.[14] "They speak the language of infinite vastness governed by law to the mind fertile in scientific hypotheses; they tell a story of gods and heroes, of loves and antipathies, of births and deaths, to the mind of the romantic poet; they become father, mother, and children, and animals all living a life like our own, to the mind of the myth maker." The facts in the field of "law in action" will speak with one voice to the research student who studies cultural processes generally, and with a different voice to the student whose main concern is law reform. This, however, is a condition which develops as research progresses. At this point it is advisable to consider the nature of some of the typical problems involved in the cultural approach.

Since ancient times it has been noticed that a correlation exists

[13] *The Possibilities of Social Study as a Science* (1931) in Essays on Research in the Social Sciences (1931) 27.

[14] Textbook of Logic (1924) 247. Quoted Cook op. cit. supra note 13 at 32.

between certain aspects of social life and geographical conditions. Thus Aristotle[15] explained the superiority of the Greeks over other races upon the basis of the middle position which the Greeks occupied between the over-civilized Asiatics and the under-civilized Gaul and Thracian. He argued that the northern races were courageous but stupid and the Orientals intelligent but spiritless, while the Greeks, who dwelt in an intermediate region were high-spirited and also intelligent. Observations of this sort have been put forward in countless numbers until, as Sorokin[16] has observed, there is scarcely "any physical or psychical trait in man, any characteristic in the social organization of a group, any social process or historical event, which has not been accounted for by geographical factors." In recent times a serious attempt has been made to give these hypotheses a more scientific form. Brunhes[17] suggests that there is a more direct correlation between geographical factors and man's first vital necessities—food, sleep, clothing, defense—than between such factors and other social phenomena. He, therefore, attempts a positive classification of the types of social phenomena which are more directly influenced by geographical agencies. These he conceives to be six in number: The shelter or habitation, the road or line of passage, the cultivation of fields and the breeding of animals, the exploitation of minerals and devastation in plant and animal life. Beyond these six essential facts social phenomena exhibit less, if any, correlation with geographical influences; still to the limited extent to which the essential facts explain, or serve to explain, the social facts by their localization and their particular forms, we have the right to connect the social facts with human geography.[18] He utters one final word of caution: "Between the facts of the physical order there are sometimes relations of causality; between facts of human geography there are usually only relations of connection. To force, so to speak, the bond which connects phenomena with each other is scientifically false; and there will be great

[15] Politics, vii, 7. (Jowett's Translation 1885).
[16] Contemporary Sociological Theories (1927) 100.
[17] Human Geography (1920) 46–51.
[18] Op. cit. supra note 17 at c. viii.

need of the spirit of criticism which will enable one to see clearly the many cases where connection is accidental and not causal."[19] Brunhes' approach to the problem of relationship of social phenomena and geographical factors is typical of the reformed spirit of investigation which now marks the whole subject.[20]

Is there a connection between legal phenomena and geography? Montesquieu[21] and Buckle[22] thought that in some respects there was a direct connection, although their chief emphasis was on the factor of climate. Randall,[23] in what appears to be the only existing systematic study of the subject, offers a number of interesting suggestions for further investigation, and he concludes that it can be fairly claimed on behalf of the geographic method that it is likely to throw considerable light upon the phases of culture that are embodied in legal rules. Certain obvious and direct relationships between law and geography have long been noted. It is clear, as Randall says, that the maritime law of Peru and Switzerland will be rudimentary; that the law of a pastoral community will differ from that of an agricultural; that of one based on tree cultivation from one based on corn raising; and

[19] Op. cit. supra note 17 at 593.

[20] It is interesting to note that in geography as well as in law there has been a separation of "pure" and "applied" science. "For centuries two conceptions of geography have been opposed to each other," Brunhes writes (op. cit. supra note 17 at 29); "by generalizing and perhaps stretching the facts a bit, one might be called the Greek conception, the other the Roman conception. The Greek conception was loftier and truer. The Greek geographers, Thales of Miletus, Eratosthenes, Hippocrates, and Aristotle, were philosophers. They had a general, philosophic conception of the physical universe and they sought *before everything else* to work out the natural succession of phenomena and how these phenomena were subordinated to each other. Then came the Romans with their utilitarian spirit; their geography was *practical.* They established itineraries, and composed topographical dictionaries; they were especially dominated by commercial interests, by administrative problems, or by ambitions of conquest. From that time general and speculative geography was neglected; the spirit of geographical science and the taste for it were lost. Only a few men, as rare as they were farseeing, strove to preserve the scientific point of view in geography. (Author's italics.)

[21] The Spirit of Laws (Bohn. ed., 1909) Bk I, c. iii.

[22] 1 History of Civilization (World's classics ed. 1925) c. ii.

[23] Law and Geography (1918) 3 Evolution of Law 198. See also Wigmore 3 A Panorama of the World's Legal Systems (1928) 1133; idem, *A Map of the World's Law*, 19 Geographical Review (1929) 114; Dubbs, *The Unfolding of Law in the Mountain Region* (1926) 3 Colorado Magazine 113.

that of industrial and trading communities from all others. Blackstone[24] pointed out that "in the Isle of Man, to take away a horse or ox was no felony, but a trespass, because of the difficulty in that little territory to conceal them or to carry them off; but to steal a pig or a fowl, which is easily done, was a capital misdemeanor, and the offender punished with death." The influence of the sea in this island of fishermen is apparent in the customary form of oath. The judges or deemsters swear to execute the laws as impartially "as the herring's backbone doth lie in the middle of the fish." Manifestly, a multitude of such relationships exist between law and geography. The task remains, however, to collect enough of such instances to decide whether or not any general principle can be deduced from them. In addition, the concept itself may be extended in at least three directions. First, we may endeavor to understand the more subtle influences of geography upon the law, although, of course, here the need for care cannot be too over-stressed. An instance of an attempt in this direction is Pound's[25] hypothesis of the pioneer influence on American jurisprudence. Pound has shown that the two great generic forms of human habitation, urban and rural life, have had a direct effect upon the development of American law. Our American common-law polity reflects the spirit of the pioneer; "it presupposes," he writes, "an American farming community of the first half of the nineteenth century; a situation as far apart as the poles from what our legal system has had to meet in the endeavor to administer justice to great urban communities at the end of the nineteenth and in the twentieth century." Second, we may think of the relationship of law and geography not merely as the connection of law with its physical surroundings, but in the broader sense of the relationship of law and the environment. Under this classification we may subsume the physical surroundings and what Graham Wallas[26] has termed the "social heritage" and Briffault[27] "traditional heredity."

[24] Quoted Semple, Influences of Geographic Environment (1927) 40.
[25] The Spirit of the Common Law (1921) c. v.
[26] Our Social Heritage (1921) 14 et seq.
[27] 1 The Mothers (1927) 23 et seq.

By the term "social heritage" is meant the habits, knowledge and expedients which are handed down from one generation to another by the social process of teaching and learning. All those mental characters which are specifically human, as Briffault has shown, are the products of traditional heredity. "Every step," he writes, "in human thought and feeling is strictly determined by what went before. The boldest speculation of the thinker is bound within narrow limits by the thought of his predecessors, and is the direct outcome of an evolution which goes back in unbroken continuity to the first flickerings of the human mind. Here, as elsewhere, evolution is gradual modification, not creation. No human sentiment, no idea, no institution has ever been created and made its appearance suddenly and 'de novo.' " All the social techniques and arts, all the folkways and institutions, which are manifested in society are also a part of the social heritage. "If the earth were struck by one of Mr. Wells' comets," Wallas writes, "and if, in consequence, every human being now alive were to lose all the knowledge and habits which he had acquired from preceding generations (though retaining unchanged all his own powers of invention, and memory, and habitation), nine-tenths of the inhabitants of London or New York would be dead in a month, and 99 per cent. of the remaining tenth would be dead in six months. They would have no language to express their thoughts, and no thoughts but vague reverie. They could not read notices, or drive motors or horses. They would wander about, led by the inarticulate voices of a few naturally dominant individuals, drowning themselves, as thirst came on, in hundreds at the riverside landing places, looting those shops where the smell of decaying food attracted them, and perhaps at the end stumbling on the expedient of cannibalism." Law is both a part and a product of the social heritage. This is a relationship which today is explored only incidentally and unconsciously as an aspect of historical research; it still remains, so far as its importance to the law is concerned, to be brought within the realm of social theory and related to social process generally. Finally, we should remember that the problem of the relationship of law and the environment is but a phase of the problem of social causation;

it is one of the many doors which open on this still relatively undeveloped field.

A characteristic of American, as distinguished from European, social thought is its antipathy to philosophical analysis; nevertheless, it is difficult to see how the metaphysical questions connected with the problem of social causation can be safely ignored. At the very threshold we must determine whether the cause and effect concept has any meaning. Russell[28] once took the position that "the word 'cause' is so inextricably bound up with misleading associations as to make its complete extrusion from the philosophical vocabulary desirable." He argued that the idea of "cause" broke down under philosophical analysis, and that, in fact, laws of probable sequence of the type contemplated by Mill, though useful in the infancy of science, were displaced by laws, the constancy of which consist in a "sameness of relations" or a "sameness of differential equations." This position had the historic support of Galileo in the *Discorsi*, who in discussing the law of falling bodies said: "It does not seem to me advantageous now to examine what the cause of acceleration is."[29] Similarly, Newton remarked: "I have not yet been able to determine from the phenomena the cause of these properties of gravitation, and I do not invent hypotheses (*Hypotheses non fingo*). It is sufficient that gravitation exists, that it acts according to the laws we have formulated, and that it is capable of explaining all motions of heavenly bodies and of the sea."[30] It may be said of this position that it is an admirably cautious one; but it is nevertheless a position which can be taken only by those advanced sciences which have been successful in the discovery and establishment of laws asserting invariant relations. Furthermore, it ignores, rather than solves, the question whether or not the statement "A causes B" can be interpreted so as to possess satisfactory meaning. On this latter point, philosophers are still hopelessly at odds. The influence of Hume[31] has persisted, but the present dispute oscillates

[28] Mysticism and Logic (1925) 180.
[29] Quoted Weyl, The Open World (1932) 36.
[30] Ibid.
[31] A Treatise of Human Nature (1920) Pt. III.

around Russell's[32] recent assertion that causal laws express nothing but regularities of sequence. This view has much to recommend it, but it appears to lead to certain paradoxical conclusions. Thus night will be the cause of day and day the cause of night. Similarly, the blowing of the factory whistle is the cause of the position of the hands of the clock when the men begin to leave the factory for dinner and the blowing of the whistle in a New York factory causes both the departure of the men from that factory and also the departure of the men from factories in Philadelphia and in Baltimore, and conversely.[33] These difficulties seem to arise because of the present inability to isolate a causal characteristic. However, it does not follow that the statement "A causes B" is without meaning or that social scientists should abandon the concept of cause. It means only that the social scientist should formulate as precisely as may be what *he* means by causation. It may not be possible for him to isolate the causal characteristic, but he may be able to discover sequences entitled to be called "causal."

If the social scientist is to continue to employ the notion of cause—and no valid reasons suggest themselves why he should not—it seems advisable to use it in the sense suggested by Keynes: "We wish to know," he writes,[34] "whether knowledge of one fact throws light *of any kind* upon the likelihood of another. The theory of causality is only important because it is thought that by means of its assumptions light can be thrown by the experience of one phenomenon upon the expectation of another." This appears to be the most general statement of the problem confronting the social scientist. It is in this sense that Weber[35] sought to ascertain the extent to which Protestantism took part in the qualitative formation and the quantitative expansion of capitalism throughout the world. The concept also indicates the importance of

[32] The Analysis of Mind (1921) c. v. A valuable discussion of the present difficulties is contained in Stebbing, A Modern Introduction to Logic (1930) c. xv.

[33] Stebbing op. cit. supra note 32 at 283; Broad, The Mind and its Place in Nature (1925) 455.

[34] A Treatise on Probability (1921) 277.

[35] The Protestant Ethic and the Spirit of Capitalism (1930).

focusing the inquiry, in the first stages at any rate, upon specific institutions or patterns. To state that technological adaptation or economic pressure is the cause of social change is too vague to have any meaning, unless the particular institution which is the subject of change is pointed out. It is perhaps of greater importance, as Cohen[36] has suggested "to recognize that social science is for the most part concerned not like physics with laws expressing the invariant repetition of elements, nor with laws of individual psychic events, but with laws about the relation of very complex patterns to one another." Undoubtedly this is one of the ideal goals of the social scientist; it represents also perhaps the only possible achievement in this field. We may look at social change in two ways: (a) We may consider the relation of one complex pattern to another, and from this position hope to formulate laws expressing social causation; (b) but social change is not solely the result of the relation of one complex pattern to another: it may be the product of a simple event, such as the invention of the cotton gin. Since we can never foretell when events of this latter class will occur, causation in this sense must always be historical; it can not be the basis, until the event has occurred, for prediction as to the future.

The intimate connections between this problem and law are too obvious to dwell upon. Law is subject to incessant change, and in turn itself exercises a determining influence upon most or all of the major patterns of society. There is here a real task for both the legal student and the social scientist in isolating the major determiners of legal change, and in evaluating the status of law itself as a causative social factor.

As a final example of the kind of problems suggested by the cultural approach, it is enough to mention the question of the nature and function of the regulative principles by which society is sustained.[37] Here are raised the general problems of the place in society of law and custom, fashion, habit, religion and morals; the nature of sanctions; the basis of authority; the character of

[36] Op. cit. supra note 11 at 361.

[37] The most satisfactory discussion of this question from the sociological position is contained in MacIver, Society, Its Structure and Changes (1931) 248, et seq.

primitive law; and the application of the social codes to individual conduct. These and similar questions are as properly within the scope of legal theory as within that of sociology. All of them must be answered before we can hope to have a satisfactory science of law; but they are in grave danger of being overlooked entirely by the legal theorist unless the present emphasis upon legal reform is qualified so as to stimulate the study of the cultural aspects of law, irrespective of the question of its possible benefit to the administration of justice.

The view advocated in this paper is the simple one that a science of law must be founded on an adequate theory of human society, and that the construction of that theory is in part a task within the domain of the legal theorist. Today, as I have attempted to show, legal study does not exhibit the characteristics of a social science; it is a technology. By its emphasis upon technological ideals it is overlooking a set of problems which must be solved before we can ever have a fruitful theory of law or a sound applied legal science. I have said nothing here with respect to the question of method, as it has seemed to me that no exclusive position is warranted by our present knowledge. We cannot rule out as inherently imperfect the comparative, the statistical or any other method; neither can we say that a particular method shall be employed to the exclusion of all others. The general method, until more satisfactory tools are developed, will be that of hypothesis and verification, with such assistance as the more individualized methods can render.

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Linguistic Aspects of Science

BY

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I

SCIENTIFIC method interests the linguist not only as it interests every scientific worker, but also in a special way, because the scientist, as part of his method, utters certain very peculiar speech-forms. The linguist naturally divides scientific activity into two phases: the scientist performs "handling" actions (observation, collecting of specimens, experiment) and utters speech (report, classification, hypothesis, prediction).[1] The speech-forms which the scientist utters are peculiar both in their form and in their effect upon hearers.

The forms of the scientist's speech are so peculiar in vocabulary and syntax that most members of his speech-community do not understand them. If one wants to read an English treatise on mechanics, it is not sufficient that one be a native speaker of English: one needs also a severe supplementary training.

The effect upon hearers of the scientist's speech is even more remarkable. In a brief utterance the scientist manages to say things which in ordinary language would require a vast amount of talk. He "manages to say things"—that is, his hearers respond uniformly and in a predictable way. Indeed, their response is even more uniform and predictable than is the hearers' response to ordinary speeches. This appears strikingly in self-stimulation:

[1] This division is the natural one for a linguist; it is necessary also for psychology; see A. P. Weiss, *A theoretical basis of human behavior*, second edition, Columbus, 1929, p. 307.

by the use of scientific speech-forms (notably, mathematical calculation) we may successfully plan procedures which we could not plan in ordinary language. Moreover, the scientist manages to say very complex things: if his statements were put into ordinary language, the phrases would become so involved (especially in the way of box-within-box syntactic constructions) that the hearers would not "understand"—that is, they could make no conventionally adequate (uniform and predictable) response. Also, it is a remarkable fact, and of great linguistic interest, that many of the scientist's utterances cannot be made in actual speech, but only in writing: their structure is so complex that a visual record, for simultaneous survey and back-reference, is indispensable.[2]

It is evident that the speech-forms of the scientist constitute a highly specialized linguistic phenomenon. To describe and evaluate this phenomenon is first and foremost a problem of linguistics. The linguist may fail to go very far toward the solution of this problem, especially if he lacks competence in branches of science other than his own. It is with the greatest diffidence that the present writer dares touch upon it. But it is the linguist

[2] It is necessary that we understand that writing is not "language," but a device for recording language utterances. The utterances that are made in any one language consist structurally of a finite number of recurrent units, and this on two levels. In the first place, the utterances in a language consist of various combinations of smallest units (*words*) that can be spoken alone. Of these there are in any one speech-community some tens of thousands. Hence a language can be replaced by some tens of thousands of unit signals (say, visual marks), each of which replaces the utterance of one word. This is the principle of Chinese writing. In the second place, the words in any one language consist of various combinations of a few dozen typical sounds (*phonemes*). Hence a language can be replaced by a few dozen unit signals (say, visual marks), each of which replaces the utterance of a phoneme. This is the principle of alphabetic writing. All writing is a relatively recent invention, whose use until yesterday, was confined to a few favored persons. A system of writing opens the possibility of graphic notations that cannot be successfully paralleled in actual speech. This is true because visual symbols possess characteristics that are foreign to the sound-waves of speech: chiefly, they provide an enduring instead of an immediately vanishing stimulus, and offer possibilities of arrangement (tabulation) that cannot be matched in the succession of acoustic stimuli. Graphic notations that cannot be matched in actual speech have arisen in the case of classical Chinese (which is unintelligible in modern pronunciation, but is read and written by Chinese scholars; cf. B. Karlgren, *Sound and symbol in Chinese*, London, 1923) and in the case which here interests us, of mathematical and allied notations.

and only the linguist who can take the first steps toward its solution; to attack this problem without competence in linguistics is to court disaster. The endless confusion of what is written about the foundations of science or of mathematics is due very largely to the authors' lack of linguistic information. To mention an elementary point: the relation of writing to language appears in a peculiar and highly specialized shape, as we have seen, in the utterances of the scientist. It would be easy to show by examples that a student who blunders as to the relation of ordinary writing to language cannot be expected to make clear formulation of this complex special case.[3]

Among the confusions of this sort there is one which runs deepest,—so deep, in fact, that some non-linguistic students have discovered it for themselves and in more recent discussions have managed to avoid it: the naïve transition from speech to inner goings-on. In everyday life, upon the instant that a speech strikes our ears, we respond only to its meaning; that is, we very properly respond to the speech upon the basis of the normal habits of language, and we do not stop to examine the sounds of the speech or to analyze its grammatical structure. When someone says "I'm thirsty," we all say "He is thirsty," and proceed to treat him as one in need of drink. The linguist alone, when acting in his professional capacity, responds to the structure

[3] A less elementary point of the same sort appears in the "heterological" contradiction: An adjective which describes itself is *autological* (e.g., *short* is autological, since the adjective *short* is actually a short word). An adjective which is not autological is *heterological* (e.g., *long* is not a long word). Is the adjective *heterological* heterological? If it is heterological, it describes itself and is therefore autological. If it is autological, it does not describe itself and therefore is heterological.—The fallacy is due to misuse of linguistic terms: the phrase "an adjective which describes itself" makes no sense in any usable terminology of linguistics; the example of *short* illustrates a situation which could be described only in a different discourse. E.g.: We may set up, without very rigid boundaries, as to meaning, various classes of adjectives. An adjective which describes a phonetic feature of words is *morphonymic* (e.g., *short*, *long*, *monosyllabic*). A morphonymic adjective which describes a phonetic feature of itself is *autological*. A morphonymic adjective which is not autological is *heterological*. The adjectives *autological* and *heterological* designate meanings of adjectives and not phonetic features; hence they are not morphonymic.—Contrast the following sensible discourse: A *hakab* is a word that ends in a bilabial stop (p, b). A word that is not a hakab is a *cowp*. The words *hakab* and *cowp* are hakabs.

(phonetic, grammatical, lexical) of the utterance. In the matter which here concerns us, this difference is important, because the speech-forms of scientific discourse are not a part of everyday speech and do not serve as everyman's signals for simple actions such as drawing a cup of water. When the scientist utters one of his queer speeches, the non-linguist cannot summarize the situation by any such unambiguous résumé as the phrase "He is thirsty." Lacking this, he has developed, in the course of history, a substitute formula. If the scientist says "Two n-spaces in an (n + 1)-space have at least an (n − 1)-space in common," the non-linguist says of the scientist "He has a *concept* of a space of n dimensions, a *concept* of a space of n plus one dimensions, a *concept* of a space of n minus one dimensions," and so on. This formula is an uncritical reproduction of the everyday formula of the type "He is thirsty." When anyone says "I'm thirsty," we at once behave on the premise that certain things, invisible to us, are happening inside him. Physiology confirms this supposition. Accordingly, when the scientist makes his utterance, we naïvely proceed to make a parallel supposition, to the effect that, inside him, invisible to us, and distinct from his mere speech, there is occurring a Something (a "concept") which is "represented" or "communicated" (in one-to-one correspondence) by his speech, just as a speaker's condition of thirst (an unmistakable physiological process) is "represented" or "communicated" by the speech of the thirsty man. Physiology has never confirmed this supposition, and, indeed, one does not expect her to, but resorts to the claim that the Concepts are invisible, intangible, and, in short, non-material entities.

Although I am one of those who believe this claim and the entire baggage of mentalism to be empty and useless, I need not here attempt to re-argue the matter, for the Concept hypothesis, even if one accepts it, is totally irrelevant to the workings of scientific speech. It is one of the very first rules of scientific discourse that the hearers respond only to the (spoken or written) statements of the scientist, and that they do not respond to anything that may be going on inside him; in particular, they do not respond to any supplementary statement about goings-on inside

the scientist. Science, we say, is "objective." And when his turn comes to speak, the scientist knows that he may not expect any allowance to be made for his "subjective" adventures,—for anything that may be going on inside him,—and that his audience will respond only to the exact stimulus-value of his words, determined according to well-fixed conventions. A scientific utterance differs from an ordinary utterance in this, that the conventional response does not allow of variations based upon goings-on inside the person of the speaker or of the hearer. To seek the foundations of scientific terminology in the world of Concepts is to create confusion and paradox. In sum, the goings-on within the scientist,—regard them as physiological or as "mental,"—are explicitly ruled out from the conventional response to scientific speech: the scientist's "concepts," "notions," "ideas," stomach-aches, and what not play no part in the communication, and what is spoken is accepted only at face value.

II

A typical linguistic situation is that of a person who has been off exploring and returns with a speech: "There is ə steep cliff at the far side of that rise in the land," "Cranberries grow in that marsh," "There are some good apples in the pantry," and so on. We call such speeches reports, and we may also call them predictions, for they imply that if you make such and such movements you will incur such and such a stimulus. We respond to them as we do to other speeches, in various ways, and often with error and confusion. It is for the good of the community, in a simple biologic sense, that this error and confusion be kept to a minimum. The limiting case would be a report so phrased that, as to any biologically or socially relevant features of the response, every member of the community would respond in exactly the same way. In any community, some reports come nearer than others to this limiting case. Some such may be preserved in the way of tradition. In a highly organized community there are specialists in such reporting and in receiving such reports, and the traditional body of these reports is held fast in written form, constituting the "body" of science, and there is developed, as time goes on, a special

vocabulary and phraseology suited to the optimum sureness of report,—the terminology of science.

In the phase of observation and experiment we are aware of our limitations and illustrate them by saying that we have no absolute guarantee that the sun will rise tomorrow; in the same way, there is no scientific report to which all qualified persons (and who are they?) will respond in exactly the same way. On the other hand, we probably cannot draw any sharp line between ordinary and scientific reports. Hence, lacking an absolute definition of science, we can say with sureness "This is science" and "This is not science" only part of the time, and we can describe the characteristics of science but cannot sharply define them. One of the most important of these characteristics is to be found in the linguistic phase of science: science uses language-forms to which all qualified persons respond in as nearly as possible the same way. Qualified persons are those who, under the division of labor, have been trained to respond to the language-forms used in science, for these language-forms differ from those of everyday life and most persons cannot respond to them at all. The linguist is interested in studying the origin of these language-forms, their relation to everyday language-forms, their structure, the way the scientist uses them, and so on.

The language-forms which serve us best in this matter are the number-words of everyday speech (*one*, *two*, *three*, . . .) and expressions defined in terms of these.

It is at this point that the non-linguist will make a detour. Suppose that we found the word *elephant* to play an important part in all scientific discourse. The non-linguist would pay scant attention to the word itself, but would insist upon a thorough study of elephants, in the way of zoology, before he went on, and if this could not be carried to a conclusion, would insist upon the paradox of our uncertainty about the ultimate nature of elephants, which conflicted with the usefulness of the word *elephant* in scientific discourse,—an evil paradox, indeed, since, uncertain about the very nature of scientific discourse, we cannot hope to rationalize our knowledge of elephants.

Now, we are not concerned with elephants, and must take them

for granted and leave the zoologist to study them. We cannot study everything at once, and must accept with humility the vicious circle of science, as of life, finding our test of success in our ability to predict and to create. We need ask only how the word *elephant* came to play its part in scientific discourse, what service it does, how it works along with the other terms of scientific speech, and so on.

In every speech-community there are certain speech-forms toward which our response is relatively constant and uniform. The physicist, physiologist, psychologist, and anthropologist who study the situations in which these speech-forms are uttered and the responses which a hearer makes to these speech-forms, may not find them simple at all, but that is none of our present concern. We are concerned merely with the fact that our responses (including further speech) to certain speech-forms are relatively constant and uniform, and that these speech-forms constitute the basis of scientific speech. Here again, there is no absolute boundary: the more constant and uniform our use of a speech-form, the better suited is this speech-form to scientific reporting. Other things being equal, the more narrowly we restrict a scientific report to speech-forms of maximum response-uniformity, the better will be the success of that report.

III

Limiting our consideration, for the present, to the English language, let us examine a few of the speech-forms which call forth the most uniform response and have passed, accordingly, into the special dialect of the scientist.

One is the word *not*. It is seldom uttered alone, but appears usually in phrases, such as *Don't!—Mustn't!—There aren't any berries over there*.[4] To state the meaning of this word is a problem for the physiologist, psychologist, and anthropologist. We

[4] The parallel phrase without *not* of the last example is *There are berries over there*. The plus of the word *any* in the example illustrates the special twists, often quite bizarre, which we find in different languages; in setting up rules for scientific speech, we exclude these features or try to make them innocuous. A striking example is the troublesome adjectival *no* of English (*No cat has nine tails*, etc.).

should be wrong, however, if we quit work until these students came to a satisfactory definition of the English word *not*. They would very properly retort by laying down their tools and saying "We cannot go ahead until you have given us the foundations of science. We cannot give you a definition of the word *not* until you have established a terminology in which definitions are to be given." It is evident that we must content ourselves with a provisional definition which will be improved or replaced as the psychologist, anthropologist, and so on make progress; they, in turn, will make revision of their results whenever this is demanded by our general principles of scientific discourse. Nowadays we should define the word *not* as the linguistic *inhibitor* in our speech-community.[5] This is plain in the most elementary of the phrases containing it, the inhibition *Don't!* In the scientist's speeches, whose typical form is prediction, the word *not* inhibits the hearer who might otherwise move in a certain direction or utter certain speech-forms. The two statements,

There are berries behind that hill;
There aren't any berries behind that hill,

are such that a hearer cannot make any one type of conventional response to both at the same time; thus, he cannot at the same time go behind the hill with his berry-basket and stay at home. The utterance, in a phrase, of the word *not* produces a phrase such that simultaneous parallel response to both this phrase and the parallel phrase without *not* cannot be made Such a word can exist because the human body occupies only one continuous region in the space-time world. We observe especially that the human body is condemned to incessant travel, always in one sense, along the axis of time The chief point for us, however, is this, that although many men now living can improve upon the above description of the word *not*, none can give a rigid and satisfying definition for our present use, simply because any definition, if rigid, presupposes a scientific vocabulary which in turn presup-

[5] *Inhibition* is here a physiological term (Pavlov): all English-speaking persons have been trained so that hearing the word *not* inhibits them in certain definable ways.

poses the word *not*. No matter how we describe the scientific vocabulary, we must begin with a set of undefined terms.

Mathematicians have long recognized this;[6] a linguist will add merely a comment on the nature of the undefined terms. The undefined terms could be created by a process of demonstration. This is the process by which a child learns to speak. The inventor or transmitter of the scientific terminology would repeatedly perform handling actions (such as touching or moving certain objects) and at the same time utter the undefined technical term. He would repeat this until his listeners were able to utter the appropriate technical term whenever the demonstrator performed the handling action, and to perform the appropriate handling action whenever they heard the technical term.[7] If this were done, the scientific terms would actually constitute a "language." Ordinary languages are acquired by infants who have no earlier speech; this language would be acquired by persons who already spoke some ordinary language. It would differ from ordinary languages also in the smaller range and greater regularity of its vocabulary and syntax. It would deserve the name "language," however, because its speech-forms would be defined for each speaker in terms of handling actions, and not in terms of another language. For obvious reasons, such as the abstract character of the meanings, we create and transmit scientific speech-forms not by demonstration, but by explanation in terms of an ordinary language, such as English. Therefore the speech of the scientist constitutes not a "language," but a technical dialect within an ordinary language. The logistician who calls his system of symbols a "language" is using a dangerous metaphor. When he tells us that the system is complete in itself, that the symbols are to be defined only by their interplay within the "language," this may mean one of two things. It may mean that the hearer (reader) is to respond to the symbols in such ways and only in such ways as have been agreed upon. This is a self-evident demand. It may mean, however, that the author believes that,

[6] This recognition appears in an admirable way, both explicitly and in the structure of the discourse, in O. Veblen and J. W. Young, *Projective geometry*, Boston, 1918.

[7] On demonstration, see Weiss, *op. cit.*, 21.

by this fiat, he can wipe out his own English explanation. If so, he is mistaken. The hearer (reader) must in one way or another be trained to respond to the symbols. A treatise written entirely in logistic symbols would be unintelligible to readers who had not been told the meaning of the symbols.

When we have introduced the word *not*, we have created two classes of statements. Once a statement has appeared in any given discourse, then there cannot appear in this same discourse a statement differing only by the presence (plus or minus) of the word *not*. There must be some additional difference, either in the words of the two statements or in their syntactic structure or position (e.g., one of them may be part of a larger statement). Thus we have, in any discourse, statements which have been made (within this discourse they are *true* statements) and statements which have been excluded (for this discourse they are *false* statements). A new statement must be tested before it can be added to a given body of discourse. It dare not belong (directly or by implication) to the excluded group. If it has not been excluded, it may turn out, under the permanent conventions of our terminology and the temporary conventions of this particular discourse, to be a mere re-wording of a statement or statements (or parts of such) which are already in the discourse; then it is already *true* and *implied*. Or it may be indifferent, neither excluded nor implied; in this case we may add it to our discourse and make it true, or exclude it and make it false (either of these is done by an *assumption*), or we may leave it alone; in actual practice, what we do depends upon the stimuli provided by our object of study (observation, experiment, verbal record of earlier studies, etc.). Even discourses that concern probability are subject, in the first place, to this dichotomy: it comes before all questions of content.

IV

Systems by which English-speaking persons may be taught to understand and to utter scientific speech could be constructed in various ways. I have chosen the word *not* as an example of an undefined term taken over from everyday speech, because one would probably make this choice. In the same way, other unde-

fined terms have to be created,—such, for example, as a word in the sphere of the English words *and*, *or*, *if*; a word for equality ("A may in all discourse be replaced by B"); a word for class; for appurtenance to a class; and so on. There is one set of terms, however, defined and undefined, which deserves mention. In every community children are taught to recite certain words in a fixed order; these words have no meaning, outside of the fact that there exists this conventional way of reciting them.[8] These are the cardinal numerals. The tenuity of their meaning makes them ideal for scientific discourse. Nevertheless, this meaning involves certain features of man and his world, such as the linear character of our movement in time. We cannot escape some undefined terms of the character of *before* (or *predecessor* or the like). The cardinal numerals have a variety of uses in ordinary speech. The simplest is that of putting sets of objects into one-to-one correspondence without actually confronting them. Since in this operation human beings show great uniformity of response, it plays an important part in science; probably every science that is to speak in numerical terms will have to presuppose the existence of distinct objects and our trick of assigning objects to classes, often in arbitrary ways,—a long chapter of psychology and anthropology.

In some languages the cardinal numerals are said to be very few, e.g. three or four. In English and many other languages they are systematized, by features of morphology and syntax, in such a manner that, given any numeral word or phrase, a hearer can utter a numeral word or phrase in a stated before-and-after relation to the given numeral; for instance, we can (uniformly and predictably) name the immediate successor of any cardinal numeral. There is nothing mysterious about this, since all that is required is a verbal habit: the phrase *immediate successor of . . .* would suffice. This enables us to utter discourses of the familiar type:

If a positive integer is blue, its immediate successor is blue;
The first positive integer is blue;
Every positive integer is blue.

[8] This is not strictly true; the numeral *thirteen*, for instance, has another phase of meaning in our superstitious response; however, these non-serial phases of meaning are easily eliminated for scientific use.

Our certainty that the combination of the first two statements will evoke the same responses, so far as scientific speech is concerned, as will the third statement, is due not to "innate judgments a priori" or the like, but simply to the fact that we have made certain conventions (acquired certain habits) as to the use of words. A positive integer is by definition a word or phrase which has a definite place in the recitation of number-expressions (*one, two, three, . . .*). We learn to begin this recitation by saying *one*; then we learn to recite up to *twelve*; we learn the place also of *hundred, thousand, million.* Other words have components which indicate their place in the recitation, in part with irregularities of form: *thirteen, twenty, thirty, billion, trillion.* The rest are phrasal combinations which explicitly state their place in the recitation: *twenty-one, two hundred sixty-four*, etc. Whatever positive integer we may name, a speaker of English can "count" up to it. However, he need not waste his time in this way, since in any actual use we are concerned only with some of the successor-relations of the integer that has been named. When we say that the English language provides an "infinity" of positive integers, we do not mean that anybody can go on counting forever; we mean only that our language has speech-forms for saying "the immediate successor of" any integer. When we say that a class is "infinite," we do not mean that any person or set of persons has made an endless series of responses; we mean only that we have agreed upon a conventional response (a *function*) to some type of stimulus (*argument*) and have by demonstration or verbal instruction fixed our agreement so well that we respond uniformly to any new stimulus of this type. The values of an argument need not be enumerable. Where it is found useful for scientific speech, we can distinguish by using *every* with enumerable arguments and *any* with non-enumerable ("every positive integer less than 13" but "any positive integer greater than 13"), leaving *all* as an indifferent word for both cases.

V

As we proceed with the use of numbers, we find operations which make new words convenient. For instance, if we use num-

bers in talking about the giving and returning of objects, we shall find it convenient to have two parallel sets of numbers (positive and negative) and a *zero* for "quits." The most serious step of this kind is the creation of *fractions*; in the enlarged system (*rational numbers*) counting is no longer possible, but it is still true that we shall unmistakably agree upon the before-and-after relation of any two numbers. We then go on to say (Dedekind's Postulate) that we will use any phrase which is so defined that this agreement can be maintained.

We have found it useful, also, to employ speech-forms (*higher numbers*) which consist of two or more real numbers in a fixed order. The terms defined in connection with real numbers will affect these higher numbers in various ways; the details will depend upon which conventions of real-number discourse we want to maintain for higher numbers. Thus, our agreements about two-placed higher numbers (*complex numbers*) are aptly summarized by the use of the phrase "square root of minus one," written $\sqrt{-1}$ or i. Three-placed higher numbers (ordinary *vectors*) are in the same way subjected to certain conventions which are aptly summarized in certain theorems about the *scalar product* and the *vector product* of two vectors. Since our space is three-dimensional (this is for the psychologist and anthropologist to explain), three-placed vectors are useful for geometrical discourse. It is surprising to see the tenacity with which writers on this subject confuse the terminology (vectors) with the geometrical application, as though we had not learned that the spatial relations of objects are not a matter of verbal agreement (mathematics), but demand observation. Given a vector $\mathbf{X} = \{X_1, X_2, X_3\}$, we define the number $[(X_1)^2 + (X_2)^2 + (X_3)^2]^{\frac{1}{2}}$ as mg $(\mathbf{X})$, and we define the vector $\{X_1/\mathrm{mg}(\mathbf{X}), X_2/\mathrm{mg}(\mathbf{X}), X_3/\mathrm{mg}(\mathbf{X})\}$ as **di X** or simply **x**. We do this because in Euclidean space, when a straight-line interval is described, in Cartesian coordinates, by **X**, the length of this interval is described by mg(**X**), and its direction by **di X**. The description is purely verbal (numerical), but the things described are certain very general characteristics of the objects in our world; since we have learned that this description is only approximate, it should be impossible to confuse

the description with the objects. To make this confusion,—to insist that a "vector" is not merely an ordered set of three numbers, but possesses also some other characteristics,—is to forego the advantage of mathematical terminology. This terminology includes, for instance, terms like *differential* and *gradient*, whose definition makes it easy to show the truth of the statement: "The gradient of a scalar function of a vector has the direction of the greatest rate of change of the scalar function." Instead of the simple and rigorous arithmetical proof, we find in our physics books a horrid little diagram, with the differentials drawn in as short lines. This diagram is more abstract than the numerical statement. By looking at the diagram and at the same time reading a not entirely numerical proof (whose rigor is hard to control), we arrive finally at the above statement.[9]

Since I am not a mathematician, I shall not attempt to carry this farther, beyond expressing the suspicion that the same confusion exists in the matter of tensors, which are perhaps to be defined as ordered sets of ordered sets of numbers (matrices); the behavior of tensors under a change of coordinates characterizes merely one of the operations that may be performed upon them, an operation useful in the application to geometry and physics, but by no means necessary (and indeed damaging) to their definition.

Mathematics is not on a level with the other sciences; it is in principle a part of every science; it is the technique of making statements in the speech-forms which elicit the most uniform response. The logical calculus and the phoneme notation of linguistics are mathematical; as numerical discourse seems to secure the greatest uniformity of response, it may be advisable to reduce such non-numerical types, in theory at least, to numerical form.

VI

A report or prediction is limited in extent. The reporter does not continue indefinitely to make speech-responses to the object. In describing a tree we do not count its leaves. In scientific work,

[9] On vectors, see E. Study, *Einleitung in die Theorie der Invarianten*, Brunswick, 1923.

the selection of useful features for report is one of the criteria of skill. Perhaps every scientific worker has had the chastening experience of hearing someone else base a useful classification or prediction upon features which he had seen and passed by as unessential. Since the scientist's report omits many verbal responses that can be made, it does not fully describe the "actual object." The non-linguist, unaccustomed to a persistent study of mere speech, jumps to the fiction of a "conceptual" object which does correspond exactly to the scientist's report. For instance, in geometry we may discuss relations which concern only one dimension; we may discuss *lines*, *curves*, and so on, mentioning only the length relations of objects and refraining from discussion of their width and thickness. If a record of observations is tabulated by dots on a chart and these dots come close to a simply definable curve, it may be useful to name this curve, as an approximate description of the placing of the dots. Such statements are verbal, not "conceptual."

In particular, the scientist's use of the number system is distressing to the non-linguist. When we ask the distance to the nearest town and receive the answer "Five miles," we know that this is not the minutely measured straight-line Euclidean distance from the tip of our left boot to the nearest point of the city limits. No one marvels at this or draws philosophic conclusions. Various degrees of approximation are used, down to the point where our handling responses, with the best instruments, cease to yield uniform results. In the speech of the scientist two conventions are useful:

(1) Earlier approximations do not limit the scope (verbal freedom) of later approximations. Having spoken of "five miles" and "six miles" (or millimeters) does not preclude our speaking, upon another occasion, of "five and one half miles." The scientist reserves the right of making free use of intermediate number expressions whenever he is stimulated to do so either by a more minute handling of objects or by a new scheme of reporting. The number system has no contiguously closed divisions: it is *dense*.

(2) The fear of future discourse does not limit the scope (verbal

freedom) of numerical discourse. In any single instance where the scientist's report says "five miles" (or "$\sqrt{2}$ millimeters," etc.), it may be that some other report, about a different object, or after new observations, or perhaps merely under a different scheme of reporting, will say "four and nine tenths miles" or "five and one tenth miles" (or "1.4142 millimeters," and so on). The scientist reserves the right of not worrying about this eventuality, and of using any number expression that is intelligible under the conventions of arithmetic (such as real numbers under the Dedekind Postulate). The number system has no contiguously open divisions: it is *continuous*.[10]

Physicists seem to be especially given to non-linguistic treatment of linguistic forms, such as attributing a mystical character to the form of a verbal report. Some, for instance, are afraid to define *force* in terms of mass and acceleration: there is a something there that does not appear in the mere equations. Finding that the act of observation alters the state of certain objects, the physicist may (legitimately or even necessarily) decide upon a form of report which takes this circumstance into consideration, but he is merely making a mistake in linguistics if he then tries to elevate this scheme of reporting into a metaphysical principle. If we said that the inside of every opaque solid is green until the solid is cut or broken or subjected to other than the usual conditions of illumination, we should have a statement (useless, to be sure) of the same metaphysical validity as this physicist's (useful and acceptable) form of reporting.

VII

The speech-forms of scientific discourse include grammatical features. Two of these are especially important: the sentence-

[10] In its everyday meaning, the term *continuous* is not particularly descriptive of convention (2); it was chosen, of course, on account of the simple geometrical bearing of this convention.

Translated into everyday language, Dedekind's postulate amounts roughly to this: We shall use in numerical discourse any word or phrase which we can define in such a way that its before-and-after relation to any rational number whatever is unmistakably determined.—It is true that at the bottom of our discourse there lie undefined words which may arise to trouble us. However, our terminology of numbers is sound enough to prevent this in all but the most unusual situations, and even in these unusual situations a realistic diagnosis will show also the cure.

form of *statements* and the structural feature of *subordination*.[11] In scientific speech these features are limited, just as are the words which scientific speech takes over from ordinary language.

In any one utterance, a *sentence* is a word or phrase which, within the arrangement habits (*grammar*) of the language, is not a part, in this utterance, of any larger phrase. A *statement*, in English, is a sentence of a particular type, defined by grammatical features: it consists of a substantive expression (*actor*) and a finite verb expression (*action*), spoken in one of certain types of order, with the falling type of final pitch.[12] The meaning common to all English statements in contrast with all English sentences that are not statements, is for the psychologist and sociologist to define. Provisionally we may say: A (full-sentence) statement is the grammatical form which insures response of the hearer. That is, whether the hearer has or has not been stimulated by a given event or by a linguistic report of this event, the statement conventionally makes him respond to this event: the hearer's actions (including speech) are thereafter such as are appropriate to a person who has been stimulated by this event. If they are not, then the speaker is not making the normal response to the statement: he has "failed to understand" it, "disbelieves" it, and so on. The convention of scientific discourse is to use only statements. A description of scientific terminology will define a very few types of statements that are to be permitted. This can be done, for English, by naming the finite verbs which are permissible: *exists*, *equals*, *implies*, *is a member of* . . . , and so on, and by naming the permissible accompaniments. Ultimately, of course, the definitions of *statement* and of the permissible forms will contain undefined terms, taken over with as good an explanation as may be, from everyday language.

In a sentence, the smallest meaningful elements of a language are not merely strung along in succession; various relations between these elements act as conventional stimuli. The meaning

[11] In linguistics the term *statement* applies only to sentences of a certain type (*John runs*). When the phrase *John runs* appears as part of a sentence (*If John runs, he will fall*), then this phrase is not a statement. In logic, however, the term *statement* is used also of expressions which are part of larger expressions; the term is so used in III, above.

[12] For sentence-types, see L. Bloomfield, *Language*, New York, 1933, p. 170.

of a succession of elements depends upon which elements appear with which (e. g., *Drink milk* is a command, but *fresh milk* is not), upon the order of the elements (*John hit Bill: Bill hit John*), upon modifications of their phonetic shape (of this there is little in English; compare perhaps *re-solve* 'solve again' with *resolve*), and upon certain phonetic features which are reserved for this use (in English, certain conventional features of stress and pitch; compare *convict': con'vict* or *It's four o'clock*, statement, with falling pitch, versus *It's four o'clock?* question, with rising pitch). The meaning of a sentence depends upon *constructions* which are definable in terms of these structural features.[13] One type is *syntactic subordination*. The grammatical function of a phrase may be the same (or within certain restrictions the same) as that of a word or phrase which appears within it. In *Drink fresh milk* the function of *fresh milk* is the same as that of *milk* in *Drink milk*. The word *milk* is the "head" of the phrase *fresh milk*, and the word *fresh* is "subordinated" to the word *milk*. Even an actor-action phrase may be subordinated (e. g., *John runs* in *If John runs, he will fall*).[14] A certain rigid minimum of this habit is taken over into scientific speech and there luxuriates to such an extent as to render this speech unintelligible in normal spoken shape and intelligible only in the written record. In short, the parentheses of logic and mathematics are not to be taken for granted, but carry over a feature of ordinary language which requires, at bottom, certain undefined terms that can be illuminated only by a serious psychological and anthropological discussion.

VIII

The speech-forms of the English-speaking scientist are not a "language," but merely a special vocabulary and phraseology within the English language. It appears, however, that a scientific discourse which most nearly insures uniform response in its own language (e.g., English) is also most easily translated into another language (e.g., French or German). This is due, in part, to the historical circumstance that scientific terms are coined

[13] *Ibid.*, 158.
[14] *Ibid.*, 194.

in one language and then translated, by explicit definition, into others. This, however, is not true of the most elementary forms. When we adopt and limit or define speech-forms such as *not*, *one*, *two*, *three*, *implies*, *exists*, for maximum uniformity of response, we come finally to anthropologic common factors: a practically equivalent adoption or redefinition of everyday words will be possible in any other language. The explanation of the undefined terms will be different for different languages, but the meanings finally reached will be the same.

In our numerals and other mathematical terms we have reached a maximum of uniformity between languages: under certain conventions the same graphs can be used for different languages. It is true that 91, for instance, which in Chinese has the arithmetically perfect speech-form "nine ten one," exhibits in English the slightly irregular shape *nine-ty-one*, and in German is "one and ninety," in French "four twenty eleven," and in Danish "one and half five-times." Under a simple convention, however, the same graphic mark, 91, is used in writing all these languages. This feature of writing is a superficial symptom of the linguistic circumstance that a number, such as *ninety-one*, is perfectly translatable, as to scientific meaning, from one language into another. The substructure of adoption and definition of everyday words and grammatical features differs for different languages, but the actual terminology and syntax of scientific discourse tends to be conventionalized,—and in the case of mathematics has already been conventionalized,—to such an extent that the difference between languages is an infinitesimal of higher order than the difference between individual speakers of the same language.

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DISCUSSION (CORRESPONDENCE)

In this section we shall publish brief communications commenting on published papers or bringing up special points of interest which can be discussed by correspondence.

MECHANISM, VITALISM AND THE ORGANISMIC HYPOTHESIS

Dear Sir:

In any science a clarification of concepts is prior to the proper and unequivocal use of terms. The purpose of this note is to clarify the concept of the organismic hypothesis in relation to the concepts of mechanism and vitalism.

The following statements are typical in exposing a certain confusion existing in biological and psychological thinking. Haldane writes: "I am not, and never have been, a vitalist, although simply because I am unable to accept the traditional mechanistic biology of the last few decades I am often regarded as a vitalist. Vitalism in any form has the same fundamental defect as the mechanistic theory of life."[1] And Köhler writes: "I wish to make the following statement expressly: "These dynamic concepts do not contain a single thought in the direction of vitalism. . . . Dynamical ideas . . . are no more the discovery of the vitalists than of the mechanists."[2] The parallelism in these two statements is not a mere coincidence. Many times have the proponents of the organismic or gestalt hypothesis asserted that they are not mechanists (meaning one thing), whereupon their opponents have taken them at their word that they are non-mechanists (meaning another thing) and labeled them vitalists, a label which was indignantly rejected by the recipients. I shall endeavor to point out the sources of this confusion with the hope of clarifying it and thus making a modest contribution towards a more exact terminology.

The organismic or gestalt hypothesis insists that the biologist and

[1] J. S. Haldane, The Philosophical Basis of Biology, p. 31.

[2] W. Köhler, Gestalt Psychology, p. 146.

psychologist[3] deal with processes that are in a constant flux in such a way that they form at any one moment a dynamic whole determined by the preceding dynamic constellation of forces and determining the succeeding one. Even the concept of force is a fluid one, a force never being a constant element in the constellation but being itself determined by the constellation. According to the opposing hypothesis, unfortunately called 'mechanistic' by the adherents of the organismic hypothesis, biological and psychological processes are composed of elementary processes each of which contributes its own characteristic function to the total pattern.

This dichotomy, organismic-mechanistic, is thus concerned with *the nature of the organization* of the pattern of forces in a certain process. On the one hand we have a dynamic whole determining at any time the constituent forces, while on the other hand we have a mosaic determined by the direction, intensity and place of the elementary forces. The mechanistic hypothesis (in this sense) has sometimes also been called 'atomistic.' Perhaps it would have been wise to use this term to the exclusion of 'mechanistic' whenever one was concerned with the nature of the organization of the forces in a certain process since 'mechanistic' was already being used in a different antithesis as will be pointed out presently.

Much older than the dichotomy of concepts just discussed is that of vitalism-mechanism. As soon as Descartes had explicitly stated his mechanistic conception of life, the opposite or vitalistic conception was also formulated explicitly. Its chief tenet is that biological processes cannot be entirely explained in terms of physical and chemical processes or forces, and that in addition to these we must assume the existence of a factor whose nature is different from them. In opposition to this hypothesis the mechanistic hypothesis holds that all biological processes exhibit no other than physical and chemical influences. This earlier dichotomy, vitalistic-mechanistic, is thus concerned with *the nature of the processes themselves* and not with their organization.

Since this is the older dichotomy we can easily see how confusion arose when a new dichotomy was labeled organismic-mechanistic. In both sets of terms the word 'mechanistic' occurs, and in each it has a different meaning. In one dichotomy, organismic-mechanistic, 'mechanistic' refers to the nature of the organization of forces and in the other, 'vitalistic-mechanistic,' it refers to the nature of the processes themselves. The resulting confusion was that anything not mechan-

[3] Lack of competence in his field prevents me from referring to the physicist.

istic could be either vitalistic or organismic. Thus it happened that men like Haldane and Köhler had to deny vigorously, if not indignantly, that they were not vitalists. Of course, they are not vitalists despite the assertions of their critics. Relative to the dichotomy vitalistic-mechanistic their hypotheses are mechanistic, that is, they do not assume a vital force over and above physical and chemical influences. However, relative to the dichotomy organismic-mechanistic their hypotheses are non-mechanistic, that is, they assume a dynamic whole and not, as Köhler puts it, a merely topographical distribution of elementary forces.

If it were not for the unfortunate double meaning of the term 'mechanistic' the confusion might not have arisen and proponents of the organismic hypothesis would not have been called upon to deny that they are vitalists. To overcome the difficulty, to remove the source of confusion and misunderstanding would demand replacing the term 'mechanistic' as the second member in the two sets of terms by two different words which are more appropriate to the underlying concepts.

I have mentioned above that the term 'atomistic' is sometimes used as the second member of the dichotomy concerned with the organization of forces, namely organismic-atomistic. Another possibility might be organismic-elementaristic. Our terminology should avoid opposing organismic by mechanistic, because the historically prior opposite to mechanistic is vitalistic.

As far as the second dichotomy, vitalistic-mechanistic, is concerned, which refers to concepts regarding the nature of the biological processes themselves, it will be more difficult, if not impossible, to find a substitute for 'mechanistic.' The term 'naturalistic' might do. However, there is no real necessity for getting rid of 'mechanistic' here, because its juxtaposition to 'vitalistic' indicates its meaning sufficiently.

Such a clarification and terminology would make it possible to characterize, for instance, a vitalistic (and similarly a mechanistic) point of view either as organismic or atomistic, or an organismic (and similarly an atomistic) point of view either as mechanistic or vitalistic. But even if no change in terminology is made,—for after all, words are merely formulae to be filled in by their context,—a clarification of concepts seems advisable to end a needless confusion.

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